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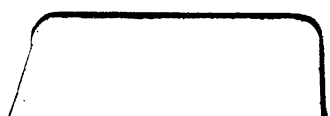
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COMPARATIVE VIEW

OF THE

HUTTONIAN AND NEPTUNIAN

Systems **SYSTEMS OF GEOLOGY:**

Millar

IN ANSWER TO THE

ILLUSTRATIONS OF THE HUTTONIAN THEORY OF THE
EARTH, BY PROFESSOR PLAYFAIR.

EDINBURGH:

PRINTED FOR ROSS AND BLACKWOOD, SOUTH BRIDGE STREET:
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1802.

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PREFACE.

THE present publication owes its origin to the "Illustrations of the Huttonian Theory of the Earth," by Professor Playfair. In that work this theory is so ably supported, its principles are placed in so advantageous a point of view, the arguments which appear to favour it are so forcibly urged, and objections so ingeniously, and often successfully obviated, that it has given to the discussion of this subject, an interest and form in a great measure new. To the Author of the present treatise, the Huttonian doctrines, whatever may be their ingenuity and novelty, appear visionary and inconsistent with the phenomena of Geology; and to a defence of them so able, and so well calculated to convey a favourable impression of the general system, he has been induced by that interest which every

one feels in the opinions he believes to be just, to endeavour to reply. Although, opposing the Huttonian Geologists, he has been anxious to avoid that spirit of hostility which too frequently pervades controversial writings: he has wished to state the arguments on both sides without partiality, and to rest the defence of the theory he supports on its intrinsic worth.

It will be admitted, that there are few questions more calculated to interest the speculative inquirer, or more fascinating, from the grandeur and novelty of the objects it brings before the mind. Nor can it be said to promise nothing but the gratification of a vain curiosity. The maxim is too well established by the history of science to require proof or illustration, that the consequences which may result from any physical discovery can never be foreseen, and that no investigation can be deemed unprofitable, which may add to our knowledge of nature. A perfect Theory of the Earth, were it established, would undoubtedly admit of the most important applications; and a succession of

theoretical discussions may not less contribute to its attainment, than the accumulation of facts. Systems, says a geological writer, are in the sciences what the passions are in the human mind : they may be the source of great errors, but they are the cause also of great exertions. Either in defending or opposing them, it is necessary to observe with accuracy, to compare and generalise ; objects apparently minute, acquire an interest and importance ; views are suggested which often lead to real acquisitions ; facts are arranged which would have remained isolated ; and relations traced which would not have been observed.

EDINBURGH, December 3. 1802.

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ERRATA.

Page 5.	line 24.	For Plutonic read	Vulcanic.
13.	14.	gneizs	gneiss.
171.	25.	time	true.

*Io veggio l'acere, Io veggio l'foco,
L'acqua, e la terra, e tutte lor misture
Venire a correzzione, e durar poco.*

Paradiso. 7.

INTRODUCTION.

THE object of GEOLOGY is to unfold the structure of the globe,—to discover by what causes its parts have been arranged,—from what operations have originated the general stratification of its materials, the inequalities with which its surface is diversified, and the immense number of different substances of which it is composed.

Researches of this kind have by some been deemed useless, from the supposition that their objects cannot be attained. We know not the history of the revolutions of the globe, but we find every where indications of their vast magnitude and antiquity. We have the most unexceptionable proof that its whole surface has been

covered with the ocean, and that every part of it has suffered change: mountains have been raised, plains levelled, islands separated from continents, and the waters collected so as to leave an elevated land. We find it difficult even to conceive causes adequate to the production of such effects; and operations so immense seem too remote, from any means of investigation we possess, to admit of being explained.

The reply which may be given to observations of this kind is satisfactory. In any subject whatever, where the *media* of proof by which a proposition is to be established, are imperfectly known, and where the proposition itself is remote from the more familiar objects of observation and research, to investigate it is always considered as impracticable; and the attempt is treated as visionary, until it be crowned with success. He who knows not the facts of geology, and the evidence they afford, may be disposed to deride its pretensions and distrust its conclusions: but a more intimate acquaintance with the subject, or even a moderate knowledge of the progress of science, will teach us to reject such narrow views. Questions apparently not more within the reach of investigation have been completely solved; and man who has weighed the planets, and measured their distances, may presume to

trace the operations by which the surface of the globe has been arranged.

On making a nearer approach to what at a distant view appears so difficult, we find that the path we have to pursue is even traced out ; that our investigations are directed by facts which derive not their support from theory, but are established by the clearest evidence. It is proved that the whole surface of the globe has at one period been in a fluid state, and that from this has originated its present arrangement. It is certain that this fluidity must have been effected by the operation either of fire or of some solvent. In our farther researches, therefore, we are nearly limited to the inquiry, by which of these means this effect has been induced ; and from the facts which apparently are within the reach of discovery, we need not despair of solving the problem. The effects of fusion, and of solution, are in general so very dissimilar, or at least the peculiarities of their action are so well marked, that from an attentive examination of the properties of minerals, and of the structure and position of the great masses of the globe, we may hope to discover to which of these they owe their origin and arrangement ; and if this discovery be established, the labours of geologists will be confined to tracing the opera-

tion of this agent in producing the phenomena observed. Their explanations may at first be imperfect, and their inductions require to be often corrected; but still such investigations must contribute to the real progress of the science, and may at length establish a perfect theory.

The fact which has been stated as the basis of all geological inquiries,—that the surface of the globe has been in a fluid state, is established by very ample evidence. In the greater number of the strata of the earth, in the most elevated, as well as in those at the greatest depths, substances are found in a crystallized state; and even many of these strata have marks of crystallization in their entire structure. Crystallization is the arrangement of the particles of a body in a regular determinate form; and it necessarily implies a previous state of fluidity which would allow these particles to arrange themselves in positions necessary to produce these forms. Many of the most solid strata contain in their substance remains or impressions of animals and vegetables; and it is obvious, that to admit of the introduction of such substances, they must at one time have been, if not in a perfectly fluid, at least in a soft or yielding state. Lastly, the general disposition of the materials of the globe, so far as has been explored, must have arisen

from fluidity, as this only could have arranged them in beds or strata parallel to each other, and preserving that parallelism to a great extent. These appearances are not partial: they extend to the whole surface of the earth, and indubitably prove its former fluidity.

It is obvious that fluidity can be supposed to be produced only in two modes. Either the solid matter must have been fused by the action of heat, or it must have been dissolved in some fluid. These are accordingly the primary propositions of the different geological theories that have been advanced. Some have supposed the surface of the globe to have been softened or melted by the operation of fire; others have ascribed the fluidity it must once have had, to solution; and as there is no fluid in sufficient abundance to have acted the part of a solvent of a quantity of matter so large, except water, *aqueous solution* has been adopted as the cause of the original fluidity of minerals, in opposition to *igneous fusion*.

These opposite systems have been distinguished by the fanciful appellations of the ^{Vulcanic} ~~Platonic~~ and the Neptunian. It would be a superfluous task to examine the different modifications of them that have been proposed. Of those which ascribe the formation of minerals to fusion, it

will probably be admitted that that advanced by Dr. Hutton is superior to any in the extent and connection of its principles, and in the adaptation of them to the explanation of the phenomena. It will therefore be sufficient to compare the Huttonian theory with the modern Neptunian system.

PART I.

Statement of the Huttonian and Neptunian Theories.

THE system which Dr. Hutton has traced of the formation of the strata of the globe, is calculated to captivate the imagination by its grandeur, and by the appearances which it exhibits of regularity and design. It presents a series of changes, each following from, or connected with the other, and so nicely adapted, as to be apparently capable of being carried on for an unlimited time. It is claimed as its peculiar excellence, " That it ascribes to the phenomena of geology an order similar to that which exists in the provinces of nature with which we are best acquainted ; that it produces seas and continents, not by accident, but by the operation of regular and uniform causes ; that it makes the decay of one part subservient to the restoration of another ; and

" gives stability to the whole, not by perpetuating individuals, but by reproducing them in succession *."

Dr. Hutton conceives that in this globe there is a system of decay and renovation, and that the processes by which these are effected have an uniform relation to each other. The solid matter of the earth is of such a nature that it must be wasted by the powers which continually operate upon it. The hardest rocks are worn down by air and water: causes which, however slowly they may operate, are constant in their action, and which therefore, in indefinite time, must be equal to the production of the greatest effect. From the figure of the surface of the earth, the decayed materials must be carried towards the ocean, and ultimately deposited in its bed. Though this transportation may be impeded by local causes, or may in general be extremely slow, yet from the declivity of the land it must necessarily take place, and may therefore be admitted as an uniformly operating cause.

It is farther assumed, that at great depths in the mineral regions an immense heat is constantly present, and that this heat operates in the fusion and consolidation of the substances deposited in these regions. It is to the action of this subterraneous fire that the formation of all our strata

is attributed. They consist of the wrecks of a former world, which have been more or less perfectly fused by this agent, and by subsequent cooling have been consolidated.

The subterraneous fire to which Dr. Hutton ascribes those effects, he conceives to operate at the same time under a particular modifying circumstance, which gives a peculiar character to his system, and frees it from many objections which were unanswerable in former hypotheses of a similar kind. This circumstance is compression. The subterraneous fire being placed at immense depths, the substances on which it operates must be under a vast pressure. This will prevent their volatilization in whole or in part; and from this circumstance it is possible to explain appearances in minerals, and qualities they possess, which would otherwise appear inconsistent with the supposition of their being formed by fire.

The last part of the Huttonian doctrine is that which regards the elevation of the strata which have been thus formed; and this likewise is supposed to be the effect of subterraneous heat. Its expansive power must occasionally be exerted with very great force; and it is inferred that this must have been the cause which elevated these strata, because no other can be assigned adequate to the production of this effect.

of a complete system, it would be more likely to possess the stability of truth.

Such strict and cautious induction, however, is not calculated to satisfy the inquiries of the theorist. It is perhaps necessary to complete the system; not merely to rest satisfied with the proof that minerals have been formed by solution, but to attempt to show how this solution, and the subsequent consolidation from it, have been effected. At least, in the present discussion, this is doing justice to the Huttonian Theory, as placing both on equal ground.

It may be observed, however, in proceeding to the statement of that modification of the Neptunian system which is generally received, that its author, Werner, has not indulged in hypothesis, but has approached as nearly to an induction of facts as the subject admits. From the positions and connections of the strata, he finds reason to conclude that they have been formed by water. From their relative situations it is evident that some have been formed prior to others, and this priority of formation he endeavours to trace and generalise. And, lastly, from their structure it is apparent that some have been chemical, others mechanical deposits, distinctions which of course ought to enter into the system.

The first principle, then, of the Wernerian theory is, that the materials of which our strata

consist were at one period dissolved or suspended in water, and that from this fluid they had successively consolidated in various combinations, partly by crystallization, and partly by mechanical deposition. Granite being the rock which composes the most elevated part of the globe, and which likewise forms the basis on which the greater number of the strata rest, is supposed to have been first formed, the different parts of which it consists, felspar, quartz, and mica, having concreated by a crystallization nearly simultaneous. This was accompanied and followed by the similar consolidation of the other primitive strata, gneiss, micaceous schistus, argillaceous schistus, porphyry, quartz, &c.

These rocks compose the chief elevations of the globe. They are never found to contain any organic remains, and of course their formation must have been prior to the existence of the vegetable and animal kingdoms.

From the period of the formation of those strata, it is supposed that the water covering the surface began to diminish in height, by retiring gradually into cavities in the internal parts of the earth. During this period, other precipitations, chiefly chemical, but in a few cases partly mechanical, continued to take place. These formed what Werner terms the intermediate strata, or strata of transition, of which some va-

rieties of limestone, shistus, and trap, are the principal. They are incumbent on the primary, and sometimes, though rarely, contain petrifications; a proof, however, that organization, or at least the existence of marine animals, had commenced with their formation.

The diminution of the water still continued to proceed, and by the mechanical action of its mass on the strata formed, it occasioned in them a partial disintegration. The materials from this source, together with the remaining part of the matter originally dissolved, by their precipitation and consolidation formed what are named the secondary strata, or the stratified rocks, sandstone, limestone, gypsum, pudding-stone, some varieties of trap, and various others. These are of a height much inferior to the former. From the predominance of mechanical deposition, they are arranged generally in horizontal beds, and they are abundant in organic remains; a proof of their formation being posterior to the full developement of the animal and vegetable kingdoms.

Of these different formations the whole surface of the globe is formed, at least with the trivial additions of the products of volcanic fire, and the alluvial beds of sand, clay, and soil, arising from the waste of the strata by the waters which run over them.

During the consolidation of these strata, rents happened in them, by which cavities of very various dimensions were formed. Into these the water, holding various matters in solution, gained access, and hence the formation of mineral veins.

Such are the systems which we have to compare. In the examination of them, we may consider them under the following divisions: First, we may investigate the probability of their principles, and the objections to which these, *a priori*, are liable: And, secondly, we may inquire how far they derive support from the structure and arrangement of the surface of the globe, and from the appearances which minerals actually exhibit.

PART II.

*Of the probability of the First Principles of the
Huttonian and Neptunian Theories.*

THE first of the series of suppositions which compose the Huttonian theory is, that the strata which form the surface of the globe, are, from the nature of their constitution, and the constant operation of the agents to which they are exposed, liable to decay ; that by being worn down and transported to the sea, they furnish materials for new strata to be formed and afterwards elevated ; and that from the decay of a former world our strata have originated.

To part of these suppositions fewer valid objections perhaps can be made than to any of the other principles of the Huttonian system. It seems to be a necessary effect arising from the nature of the solid materials at the surface of the globe, that they must be wasted by the powers which act upon them ; and many facts

have been stated, which establish such a disintegration. The immense beds of sand and gravel which are found even in elevated situations, afford in particular a clear proof of this kind, since such substances must evidently have been formed from the waste of solid rocks, and the attrition of their fragments.

Facts have, however, likewise been stated in opposition to this principle. Rocks of basaltes, and of many kinds of granite, it is observed, suffer scarcely any decay, as is evident from the sharpness of their angles. And from the testimony of history, we have it in our power to ascertain, that in many places rocks of this kind occupy the same situation which they did two thousand years back. These, and similar facts, however, are to be regarded as exceptions to the general law ; or rather, they serve to prove, that in such rocks the disintegration is extremely slow. It seems scarcely possible to deny but that it must take place to a certain extent ; and as the operations supposed in the Huttonian system are unlimited as to time, it would be in vain to contest the principle assumed.

It has also been questioned, whether the materials worn down, and conveyed by the rivers, are carried into the depths of the sea. It has been alleged, that the greater part of the matter thus conveyed is thrown by the returning tide

which they had originated. It is also observed, that in many of the most extensive strata of the earth, remains or impressions of organic substances are found, both animal and vegetable, and of course these must have existed prior to the formation of such strata.

These facts are considered by the Huttonian geologist as sufficient proof of the existence of a habitable world, from the decay of which ours has been formed. They are however equally well accounted for by the Neptunist, without admitting such a supposition. It is supposed that the existence of marine animals commenced after the crystallization of the great primary strata; and that after that period too the waters of the ocean began to diminish in height, so as to leave elevated land, on which vegetation commenced. The retreat of the ocean continued to be gradual for many ages, and during this time the secondary strata were formed. It is obvious, therefore, that the fragments of rock, the sand and gravel which these often contain, or with which they are associated, or which even in many cases compose the greatest part of their mass, might originate from the disintegration of the primary strata above the level of the sea; a disintegration to which, in this early period of their consolidation, they might even be more liable than they now are. And the origin

of the remains of marine animals, and even of vegetables, found in the secondary rocks, it is obvious are equally well accounted for on this theory, since the existence of these may have begun previous to the formation of these strata. The facts, therefore, do not prove the hypothesis of Dr. Hutton, since on a different hypothesis they are explained with equal facility.

It has been affirmed, however, that the same appearances of sand and gravel, and of marine impressions, are occasionally to be met with in the primitive strata, and that of course the Wernerian explanation is defective; for marine animals are not supposed to have existed at their formation; and it is obvious that the presence of sand and gravel are true indications of strata having existed before them.

But it is asserted, on the other hand, by Neptunian geologists, that such appearances are not to be met with in strata truly primitive, but that when they do occur in strata not of the secondary class, it is in those of the intermediate kind, or what Werner terms the rocks of transition. These, it will be recollected, are supposed to be posterior in their formation to the primary, but prior to the secondary strata, and to have been formed at that period when the existence of marine animals, or at least of some species of them, had commenced; and of course they may occasionally

be found with impressions or remains of these beings. This supposition is liable to no difficulties, and seems to follow justly from the facts. Since certain rocks, having peculiar characters, and composing the most elevated parts of the globe, are found destitute of organic remains, while in others they are abundant, does not this afford a presumption, that the former had been produced prior to the period when these beings began to exist? And if rocks are found intermediate in their characters between these, connected principally with the primary, but in general less elevated, and sometimes, though rarely, containing vestiges of sea animals, is it not reasonable to believe that these have been intermediate in their formation, and that at least the few species of those animals, whose remains are found in them, had begun to exist at the time they were formed?

It will be found that such a supposition accords much better with these phenomena, and affords a more satisfactory solution of them than the hypothesis by which they are explained in the Huttonian system. According to that system, all the strata, both those termed primitive as well as those named secondary, have been formed from materials deposited at the bottom of the ocean from the wrecks of a former world. They ought therefore all equally to contain or,

ganic remains and impressions; and it remains to be accounted for why these should be entirely wanting in many of the strata, as in gneiss, or micaceous schistus, while they are present in others. There is only one supposition by which this can be attempted. It is, that the former strata have been in more complete fusion than the latter, and that thus by the more intense heat, these remains, or the impressions of them, have been destroyed. But the explanation is contradicted by the appearances of these strata, the marks of fusion being frequently as complete in these which contain such remains, as in those which do not. Thus there are many limestones and marbles containing shells, in which the sparry structure is as perfect as it is in the primary limestone, and in which are even distributed veins of crystallized carbonate of lime; and in the transitive limestone, the parts which contain no marine impressions have no marks of more perfect fusion than these parts in which they are present, nor indeed is there any difference between them. In like manner, the primitive schistus, which has no marks of foreign bodies, has no appearances of more complete fusion than the secondary schistus, in which vegetable impressions are abundant. The supposition, therefore, of the Huttonian geologist to account for the absence of organic remains from these

strata, is not merely hypothetical, but inconsistent with the phenomena, while that of the Neptunist is so probable, that it seems to follow as a corollary from the facts.

The Huttonian explanation does not derive more support from the other general fact that has been stated in proof of the production of all our strata from the wrecks of a former world, —the association of breccias, of sand or gravel with the primary rocks. Several instances of this kind, stated by Professor Playfair are extremely doubtful. But granting that there are such appearances which cannot be disputed, they are easily explained. It is obvious that on the gradual retreat of the sea, which happened after the formation of the primary rocks, the most elevated would first be left bare; and whenever this happened, the mass of water would begin to act upon this dry land, and occasion in it disintegration. It is even reasonable to believe, though it is not necessary to make the supposition, that this disintegration would proceed more rapidly immediately after their consolidation than when they had been more fully hardened by time. Whether this were the case or not, fragments of the elevated rocks must have been worn down into sand, gravel, &c. and by the direction of currents, banks of these might be deposited among the existing or forming

strata, and might either, according to circumstances, be left in a loose state, or be consolidated by the deposition of the matter still dissolved or suspended in the fluid. There is no difficulty therefore attending the fact, that such collections are found in the transitive, or even in the truly primitive strata, nor does it establish the conclusion that such strata had originated from the waste of older continents.

There is even a particular phenomenon in this part of geology, very general in its occurrence, which admits of a natural and satisfactory explanation from the Neptunian theory, while it requires the most extravagant supposition when considered according to the principles of the Huttonian system. The appearance is that of a bed of breccia incumbent on the primary strata, and covered by one or other of the secondary strata. Over the vertical strata, for example, of the primary schistus, there is frequently a bed of breccia, composed of fragments of rocks consolidated by some cement; and over this bed is a stratum of sandstone or limestone. According to the Neptunian theory, the explanation of this is very easy. Of these the schistus, from its position, it is evident must have been first formed: other primary rocks, however, had been formed at a still earlier period, and in positions more elevated. As the sea retreated

these would be left bare, and by their disintegration would afford the fragments which, deposited on the lower shiftus, formed the breccia; and this again, before the retreat of the sea was completed, so as to leave it exposed, might be covered by a deposit of sandstone or limestone.

The explanation, according to the Huttonian hypothesis, involves a supposition so extraordinary as to furnish a singular contrast with that of the Neptunian. It is supposed * that the shiftus had been formed in beds nearly horizontal, and that by an expansive power exerted from beneath, these had been elevated to the surface, and placed in a vertical position. In this situation, the bed of gravel from which the breccia is formed, had been deposited on the summit of the vertical shiftus. To admit of the formation of the horizontal strata of sandstone, it is further supposed, that the shiftus, with this superincumbent breccia, had again sunk in the ocean, and remained depressed for ages, till the materials of the sandstone were deposited on it. These materials are supposed to have been then consolidated by the central fire operating on them, even with the intervention of the deep strata of shiftus on which they are incumbent; and lastly, we are told that the whole, when thus prepared, were again elevated by a new exertion of

* Illustrations, &c. p. 52

heat. It may surely be affirmed, without farther reasoning, that suppositions so extravagant and improbable can never be real interpretations of the operations of nature.

We may on the whole conclude, that though the existence of organic impressions and remains may prove that the strata containing them have been formed posterior to the existence of the vegetable and animal kingdoms; and though the presence of breccia, sand and gravel in the strata may likewise prove that other strata had existed, from the waste of which they had been produced, yet neither of these facts establishes the conclusion, that our strata have originated from the decay of a former world; and the explanations of the Huttonian system on this subject are even less probable and satisfactory than those given by the Neptunian theory. The general proposition may therefore be admitted, that the strata of the earth are liable to waste, and that the materials are carried forward to the sea, without proving from any appearances that this makes part of a series of changes, or is a step in the succession of worlds, or that on this planet a habitable world has existed prior to the present, and from the waste of which this has originated.

The second principle assumed in the Huttonian theory, is, that the materials which are collected at the bottom of the ocean are at great depths exposed to the action of an intense heat under a strong pressure, by which they are fused and consolidated, so as to be capable of forming new strata. This principle may be considered under different aspects; and the discussion of it is important, since it is capable of affording a *direct demonstration of the falsity of the Huttonian hypothesis*.

The difficulties which attend the opinion must first be stated. How is this immense heat produced? If from combustion, the only probable source, whence is the combustible matter derived by which it is excited? Whence is the oxygen supplied, by which that combustion must be kept up? And how can it be applied to materials collected and deposited from water, when the operation of that water is at the same time excluded? These are difficulties which may be urged against this hypothesis, and of which it will not be an easy task to give a satisfactory solution.

The intensity of the heat required to produce that fusion whence our strata could have originated is beyond what it is possible perhaps for the imagination clearly to conceive. This is evident from the immense extent of these strata.

The highest mountains of the globe run in extensive chains, and these, from their connection, must necessarily have been formed at one time. It is not less evident, from the difficult fusibility of the substances either composing their entire structure, or contained in them. This may be illustrated by one or two examples.

Carbonate of lime, of which all the calcareous crystals, marbles, limestone and chalk, consist, cannot be fused by any heat which we can command, or at least can be fused only in the most minute quantity. Lavoisier was unable to melt a particle of it by the intense heat excited by a burning mirror; and Saussure, by the much more powerful heat excited by the flame of the blow-pipe, urged by a stream of oxygen gas, was able only to fuse a particle of it, so small that it required the aid of the microscope to discover it. What then, says Mr. Kirwan, must have been the heat necessary to melt whole mountains of this matter? " Judging from all
 " we at present know of heat, such a high degree could only be produced by the purest
 " air acting on an enormous quantity of combustible matter. Now Ehrman observed, that
 " the combustion of 280 cubic inches of air
 " acting on charcoal was not able to effect the
 " fusion of one grain of Carrara marble; from
 " whence it is apparent, that all the air in the

" atmosphere, nor in ten atmospheres, would
 " not melt a single mountain of this substance
 " of any extent, even if there were a sufficient
 " quantity of inflammable matter for it to act
 " upon *."

It has been attempted to lessen, if not to obviate the force of this objection. Professor Playfair has observed, that " this reasoning is not
 " applicable to Dr. Hutton's hypothesis of subterraneous heat, because it is grounded on experiments where that very separation of the
 " volatile and fixed parts takes place, which is excluded in that hypothesis. When limestone or marble is exposed to such a heat as
 " is here mentioned, or even to heat of a degree vastly inferior, the carbonic gas is expelled, and the body is reduced to pure lime.
 " The Carrara marble may require a heat of 6300 of Wedgewood to melt it in the open
 " air, where the carbonic acid gas escapes from it; but under such a pressure as would retain
 " this gas, it cannot be inferred that it might not melt with the heat of a glass-house furnace †."

This argument, that carbonate of lime may be so much more fusible than pure lime, and that therefore the subterraneous heat by which

* Geological Essays, p. 453.

† Illustrations, p. 184.

it is supposed to have been fused, may be very moderate, since it acted under a compression by which the carbonic acid would be retained, is supposed to be confirmed by an analogical fact. Dr. Black had remarked, that pure barytes is much less fusible than carbonate of barytes, since, when carbonate of barytes is exposed to an intense heat, it first fuses or vitrifies; it then begins to part with its carbonic acid, and as it does so, it returns to the solid state. It is supposed, by analogy, that the case may be the same with carbonate of lime, that when strong compression is applied, it may be fused by a much lower heat than when the carbonic acid is allowed to escape. And such a compression is supposed, in the Huttonian theory, to have been applied in the mineral regions, where the immense masses of carbonate of lime have been formed.

The fact, it may be observed, on which this argument is founded, is doubtful. In the mode of obtaining the barytic earth, by decomposing carbonate of barytes by heat, the barytes is not obtained pure; and if the experiment be made, as it usually is, in an earthen vessel, as the decomposition proceeds, the barytes acts upon the earth of the vessel, and forms with it a species of frit. The substance obtained in this way is both much less fusible and less soluble than the

pure earth obtained by decomposing the nitrate of barytes, according to the process of Vanquelin. This earth is indeed so easily fusible, that the fact cannot be admitted, that barytes, when pure, is more infusible than when combined with carbonic acid. Precisely the reverse is the case.

But were it true, the analogy thence extended to lime is not just. Carbonate of barytes, it is said, fuses before it parts with its carbonic acid; but carbonate of lime does not. No analogy, therefore, exists between them in the increased fusibility arising from the combination of carbonic acid with these earths; or there are no just grounds for supposing that lime, like barytes, is rendered more fusible by combination with that acid; and therefore there is no foundation for this argument of Professor Playfair. And even if there were, still the heat requisite to fuse carbonate of lime must be intense. In order to decompose it, and expel the carbonic acid, a white heat is requisite, and still at this temperature the calcareous carbonate is not fused; and its fusion would probably require a much greater heat than this. When we consider, therefore, the great extent of the strata of calcareous matter, it is evident, that admitting the above analogical argument to be just, which it is not, still an immense heat must have been necessary for

their consolidation, according to the Huttonian hypothesis, a heat with which that of a glass-house furnace cannot be compared.

The original objection may also be stated with equal force, with respect to other fossils in which there is not the same mode of eluding it. Quartz, for example, according to the experiments of Lavoisier, and other chemists, is nearly as infusible as lime. It not only remains unmelted in any heat a furnace can raise, but in the heat excited by a burning mirror. Now there are entire mountains of quartz; it is found in large veins in many rocks, and in others, as in granite, it exists crystallized as a constituent part. These, therefore, must have had an intense heat applied to them for their fusion; and no modification, from the presence of pressure, or any other circumstance that could have any effect in promoting the fusion of the quartz, can be imagined. The heat of a glass-house furnace, says Mr. Playfair, trusting in the impossibility of ascertaining the precise fusibility of carbonate of lime, may have been sufficient for its fusion. But there is not the same uncertainty with regard to quartz. It is calculated by Saussure, that it requires for its fusion a temperature equal to 4043 of Wedgwood's pyrometer. Glass, at a medium, requires only 30 of the same scale. From the comparison of these, therefore, we may judge of the intensi-

ty of the heat which would be necessary to fuse the strata of the earth, and how far the standard of comparison which Professor Playfair points out is just.

The force of this argument, we may estimate from the care with which the answer to the objection, from the infusibility of carbonate of lime, is repeatedly stated, and fully urged. In enumerating the difficulties attending the Huttonian hypothesis, from the intense heat which must have been required to fuse certain substances, the two examples of carbonate of lime and quartz are the most obvious, and have been generally stated together. The defender of that hypothesis, discovers an analogical argument, by which it is supposed, that the force of the objection, from the infusibility of one of these substances, the calcareous carbonate, may be obviated; and this is brought forward, strongly stated, and repeated under various forms. But the other example, the quartz, is kept out of view. No notice is taken of its infusibility having been brought forward equally with that of the calcareous matter, as demonstrating the improbability of this principle of the Huttonian doctrine. Is not this a tacit admission of the force of the objection? Where there appears a possibility of answering it, even an imperfect analogical argument, founded on a mistake in point of fact, is

not disdained. When, in another instance, therefore, it is not attempted to answer it, is not this to admit, that no satisfactory reply can be given? that, of course, the objection, from the extreme infusibility of fossil substances, of which carbonate of lime and quartz are given as the most striking examples, remains in full force? and that the anxiety displayed in obviating it, in one instance where it seemed possible to do so, is a proof that the objection is not, in the estimation even of the defenders of this system, trivial, but one which they would wish, if possible, to remove?

So far, therefore, the improbability of this principle of the Huttonian theory, that fossils have been fused by a subterraneous heat, must appear evident from the difficulties which attend it. Whether we consider the extent of the strata thus supposed to have been fused, or the extreme infusibility of the matter of which they are composed, the heat requisite must exceed, in intensity, not only any that we know, but even any that the imagination can conceive; and for the production of such a heat, no adequate cause can be assigned.

But this argument, however forcible, may still be placed in a stronger point of view. It is the peculiar feature, and as has been stated by its author, the excellence of the Huttonian

theory, that the operations it supposes are intimately connected, and are carried on in a succession to which no limits are placed. It does not account merely for the appearances which the earth at present exhibits, but unfolds a system in which the destruction and formation of successive habitable worlds are explained. In every part of it, therefore, provision must be made, not merely for the production of an effect, but for the reproduction of that effect an indefinite number of times. Indulge the defender of this hypothesis with the assumption, that there existed in the bowels of the earth a quantity of combustible matter sufficient to have produced a heat capable of having fused the present strata, and that a sufficient quantity of air was supplied to support its burning, even this will avail him little. These strata are supposed to have originated from the decay of others which existed before them of a similar kind, and which had been formed in the same manner; and they are to be succeeded by new strata likewise fused and elevated by heat. Nothing can be more incontrovertible, than that any accumulation of combustible matter, or any collection of air which can be imagined, would not, from the very principles of the system, be sufficient to maintain such operations. It is a proposition indeed self-evident, that a limited quantity could

not serve to support operations, not only immense in their extent, but unlimited in their succession.

On the other hand, Dr. Hutton cannot show that there is any process carried on at the surface of the earth sufficiently extensive to supply combustible matter for these operations. He seems to imagine that vegetation is capable of serving this purpose. "Let us suppose," says he, "the subterraneous fire supplied with its combustible materials from this source, the vegetable bodies growing upon the surface of the land : Here is a source provided for the supplying of mineral fire ; a source which is inexhaustible or unlimited, unless we are to circumscribe it with regard to time and the necessary ingredients*."

It is true that vegetation is the great source of combustible matter at the surface of the earth ; it serves as a counterpoise to the various kinds of oxygenation, decomposes their products, disengaging the oxygen, and accumulating in the matter of plants carbon and hydrogen. These two processes appear designed in the economy of nature as antagonists to each other, oxygenation consuming the combustible matter which vegetation produces, and vegetation de-oxydating the products of oxygenation. But

* Theory of the Earth, vol. I. p. 243.

there is no reason to believe that the one general process exceeds the other in the ultimate effect. It might perhaps be urged as an objection to the Huttonian theory of the formation of coal, that it derives that substance from the excess of combustible matter produced at the surface; for it would require proof that there is any such excess, or at least such an excess as to continue for an unlimited time. But at any rate it can never be pretended that from this source can be derived that immense quantity of combustible matter necessary not only to form the coal of the succeeding world, but to support that intense combustion which is required to fuse and consolidate its earthy strata.

It even admits of demonstration that no operation carrying on at the surface of the globe can furnish an uninterrupted supply of combustible matter. In the formation of the strata, the Huttonian hypothesis labours under no difficulty with respect to providing materials, because the same matter which at one period is fused and elevated, is again worn down. But there is no such succession of combustible matter: in combustion it is necessarily rendered unfit to support the same process until it be de-oxygenated; and there is no mean pointed out, or even none which can be imagined, by which this is effected.

Were even this difficulty removed, it recurs in precisely the same force with respect to the supply of oxygen, which is just as necessary as combustible matter to combustion. No accumulation of oxygen can support combustion for an unlimited time; and no source can be imagined whence a successive supply could be derived.

Professor Playfair seems to be sufficiently aware of the force of these objections, and of the impossibility of supposing the subterraneous heat to be excited and preserved by any species of combustion; and this, as limiting the discussion, is an important concession. According to all the appearances, he observes, from which the existence of a central heat has been inferred, "it is of a nature so far different from ordinary fire, that it may require no circulation of air, and no supply of materials, to support it. It is not accompanied with inflammation or combustion, the great pressure preventing any separation of parts in the substances on which it acts, and the absence of that elastic fluid, without which heat seems to have no power to decompose bodies, even the most combustible, contributing to the unalterable nature of all the substances in the mineral regions *." Again, "In a region where the

* Illustrations, &c. p. 93.

“ action of heat was accompanied with such
 “ compression as is here supposed, there could
 “ be no fire properly so called, and no combustion.
 “ This is admitted by Dr. Hutton ; and
 “ it is therefore a fallacious argument which is
 “ brought against his theory, from the impossibility
 “ of fire being maintained in the bowels
 “ of the earth. This impossibility is precisely
 “ what he supposes, and yet Mr. Kirwan’s arguments
 “ are directed not against the existence
 “ of heat in the interior of the earth, but against
 “ the existence of burning and inflammation *.”

Professor Playfair having thus admitted, in the most express terms, that the subterranean heat, supposed to be the cause of such important effects in the Huttonian hypothesis, cannot arise from any species of combustion, proceeds to explain the mode in which he conceives it may be excited. His observations on this subject must be quoted at full length. “ It is not
 “ FIRE in the usual sense of the word, but HEAT,
 “ which is required for that purpose ; and there
 “ is nothing *chimerical* in supposing, that nature
 “ has the means of producing heat, even in a
 “ very great degree, without the assistance of
 “ fuel or of vital air. Friction is a source of
 “ heat, unlimited, for what we know, in its ex-

* Illustrations, p. 182.

“ tent, and so perhaps are other operations, both
 “ chemical and mechanical ; nor are either
 “ combustible substances, or vital air, concerned
 “ in the heat thus produced. So also the heat of
 “ the sun’s rays in the focus of a burning glass, the
 “ most intense that is known, is independent of
 “ the substances just mentioned ; and, though
 “ that heat certainly could not calcine a metal,
 “ nor even burn a piece of wood, without
 “ oxygenous gas, it would doubtless produce as
 “ high a temperature in the absence as in the
 “ presence of that gas.

“ It is true, that it is not by the solar rays
 “ that subterraneous heat is produced ; but
 “ still, from this instance, we see, that there is
 “ no incongruity in supposing the production
 “ of heat to be independent of combustible bo-
 “ dies, and of vital air. We are indeed, in
 “ all cases, strangers to the origin of heat :
 “ philosophers dispute, at this moment, con-
 “ cerning the source of that which is produced
 “ by burning ; and much more are they at a
 “ loss to determine, what upholds the light and
 “ heat of the great luminary, which animates
 “ all nature by its influence. If we would
 “ form any opinion on this subject, we shall do
 “ well to attend to the suggestions of that great
 “ philosopher, who was hardly less distinguished
 “ from others by his doubts and conjectures,

“ than by his most rigorous and profound in-
 “ vestigations. ‘ May not great, dense, and fix-
 “ ed bodies, when heated beyond a certain de-
 “ gree, emit light so copiously, as, by the emis-
 “ sion and reaction of its light, and the reflec-
 “ tions and refractions of its rays within its
 “ pores, to grow still hotter, till it comes to a
 “ certain period of heat, such as is that of the
 “ sun? And, are not the sun and fixed stars
 “ great earths, vehemently hot, whose heat is
 “ conserved by the greatness of the bodies, and
 “ the mutual action and reaction between them,
 “ and the light which they emit * ?”

“ Some recent experiments seem to make the
 “ suggestions in this query applicable to an op-
 “aque body like the earth, as well as to lumi-
 “ nous bodies, such as the sun and fixed stars.
 “ The radiation of heat, where there is no light,
 “ was first rendered probable by the experi-
 “ ments of M. Pictet of Geneva †; and the
 “ only objections to which the conclusions from
 “ those experiments seemed liable, are removed
 “ by the late very important discoveries of Dr.
 “ Herschel ‡. From these it appears that heat
 “ is capable of refraction and reflection, as well

* Newton's Optics, *ubi supra*.

† Essai sur le Feu.

‡ Phil. Trans. 1800. p. 84.

“ as light, so that it is not absurd to suppose,
 “ that *the heat of great, dense, and fixed bodies,*
 “ *may be conserved by the greatness of the bodies,*
 “ *and the mutual action and reaction between them*
 “ *and the heat which they emit* *.”

We may admire the ingenuity of these observations, in which an argument, obvious and incontrovertible, is so enveloped and disguised, that it requires some discussion to place it in its clear light. To what purpose are the various sources of heat, enumerated in this reasoning? To prove that it may exist, or be produced independent of burning. This will be readily granted: But the reasoning can prove nothing farther; it can never warrant the conclusion, that heat may be ascribed to the operation of an unknown cause. If it arise not from burning, it must be concluded that it is produced by some of its other known causes, by friction, the sun's rays, some species of chemical action, or any other; and if it can be shown, that the supposed subterranean heat cannot originate from any of these causes, the objection to the hypothesis which admits it is as strong and irresistible, as if combustion were the sole origin of heat, and it were proved that it could not be the effect of that process. Now, it can be satisfactorily shown, that any of the known

* Illustrations, &c. p. 186, 187, 188.

will this diffusion be ; for although the celerity of the propagation of caloric through bodies is not precisely according to their densities, it is more nearly in that ratio than in any other. No heat, therefore, in the centre of the earth could be kept accumulated there ; its intensity must continue uniformly to diminish ; and as the Huttonian system supposes a succession of worlds formed by this subterraneous heat, of which part shall always be solid and habitable, while another part is fluid, and in a state of preparation to be raised, it is evident, that from the supposition made of great and dense bodies preserving their heat, the local temperature necessary for these operations cannot be explained. The heat producing it must come at length to be equally distributed over the whole mass, and either render it entirely fluid, or be incapable of fusing any part.

To this view of the subject, therefore, the preservation of a central heat in a mass of matter, which is the only one that has any relation to the present question, the hypothesis of Newton does not apply. That philosopher might conceive that a great body heated, and placed at an immense distance from every other, might from its greatness preserve its heat for a very long period. Or it might be supposed that a certain portion of caloric might be preserved in

view, under which this subject may be considered. Heat, it is sufficiently known, is propagated through dense bodies with considerable celerity. If a central fire, therefore, of the greatest intensity, exist, the heat must be propagated through the substance of the earth towards its surface; and this propagation ought to be such, that, even in that period of time of which we have authentic records, its effects ought to have been apparent. Yet we have no reason to believe that there is any change in the medium temperature of the globe. The climate of particular countries may be altered, from cultivation, or other local circumstances, but no important general alteration appears to have taken place; if it had, its effects must have been conspicuous, by symptoms too well marked not to indicate their cause. Nay, no change of this kind appears to have happened for a much longer period than that which man has ascertained. It may be affirmed, that the temperature which at present prevails, is that necessary for vegetation, animal life, and, in general, for all the operations of nature; nor could a habitable world like ours, have existed with a medium temperature many degrees inferior to that which now prevails. The heat at the surface, therefore, must have always been nearly the same; and, though a central heat has been ex-

isting, according to the Huttonian theory, that immense succession of time, during our world, and others preceding it, have existed, and there has been no propagation of it through the substance of the earth. If we can rely on deduction whatever from the knowledge we possess, we may rest assured, that a system involving such a supposition is false: nor would it be easy even to imagine any process of reasoning by which its falsity could be more clearly demonstrated.

If we compare the Huttonian with the Neptunian theory, it will appear evident that, according to the one, the present argument of the surface of the earth admits of any longer duration, than it does according to the other; for the principle of renovation proposed in the former, must have its power extinguished, before the waste, which it is designed to repair, is complete. The tendency to the destruction of this globe, as an habitable world, arises from the disintegration which the earth suffers; but this disintegration is acknowledged to be extremely slow. "We have mountains in this country," says Dr. Hutton, "and they are not made of more durable materials than what are common to the earth, which are sensibly diminished in their height in a thousand years. The proof of this are the

" man roads made over some of those hills. I
 " have seen these roads as distinct as if only
 " made a few years, with superficial pits beside
 " them, from whence had been dug the gravel,
 " or materials of which they had been form-
 " ed*." If, in so long a period, the disintegration
 is so inconsiderable as not to be perceptible,
 what must be required to level these mountains
 with the sea? Millions of years would not suffice.
 If the comparison be made, of the degree of celerity
 of the propagation of caloric, and the quantity of
 matter it has to penetrate in passing from the centre
 to the surface of the globe, and that of the celerity
 of the disintegration of the existing strata, as estab-
 lished by these facts, it will be evident, that, after
 stating the former as low as possible, it must arrive
 at its termination; or, in other words, the tempera-
 ture must become uniform in a shorter period than
 that which would be necessary to complete the
 disintegration of the elevated parts of the present
 land.

It is scarcely necessary to place the absurdities
 following from this principle in any other point
 of view; yet one more may be added, if possible
 more glaring than the others. The materials
 from the waste of the present world are supposed

* Theory of the Earth, vol. ii. p. 140.

to be deposited in the bed of the ocean, and to be there consolidated by the central heat. This heat, therefore, has been capable of propagating itself through the immense mass of solid matter interposed between the centre of the earth and the bottom of the sea, and in a degree so intense as to consolidate the loose matter thus deposited. Yet beyond this it has never been able to advance, even in that vast period of time in which successive worlds have been forming : In other words, this heat has been diffused through some thousand miles, to the most intense degree, but is there arrested, and cannot extend itself a few miles farther ; a circumstance worthy to be regarded as an interposition of a superior power in favour of man. It is thus that the versatile Huttonian suspends the laws of nature at his pleasure, or finds them always pliant and accommodating to the principles of his hypothesis.

From the whole of this reasoning, it appears, that under whatever aspect we consider this part of the Huttonian theory, it not only labours under insuperable difficulties, but is proved to be false. Whether we suppose a vast accumulation of heat, or of matter, capable, by combustion, or any other mode of producing it ; or whether we suppose a successive supply of heat or of matter capable of affording it, still the sup-

ply cannot be inexhaustible, and, of course, cannot support the operations it is supposed to perform, for an indefinite time. And, were it even adequate to this, such is the nature of the agent employed, that it necessarily must prove destructive of the system it is designed to repair. There is an accumulation of proof, which prejudice itself, we should be tempted to believe, could not resist, and which is more than sufficient to establish the conclusion, that whatever praise may be due to the Huttonian system as a splendid hypothesis, it has no claim to the more exalted rank of a just theory.

In this discussion on the existence of a central heat, it has not been thought necessary to notice the argument for it, drawn from the phenomena of volcanoes; it is so obvious that these arise, not from any source of this kind, but from fire excited in the volcanic mountain. Yet, as this is an argument on which some stress is laid, it may not be improper to show its fallacy by a few facts.

1st, The heat of the matter erupted from volcanoes, is not such as it must have been, were it derived from that fire which the Huttonian geologist supposes to exist in the centre of the globe. This heat is supposed to be sufficient to fuse granite, and of course quartz, which, according to the experiments of Saussure, requires a tem-

perature equal to 4043 of Wedgewood's scale. But many facts prove, that the heat communicated by volcanic fire, to the matter it throws out, seldom, if ever, equals 120 of the same scale. This is evident from shorls, and various other fossils, fusible at 100, or 110, being found unaltered in the lava thrown out,—a proof that they had never been fused; and lava itself is fused or vitrified at a temperature below 40. Such a heat, therefore, can bear no comparison with that supposed by the Huttonian geologist to exist in the subterranean regions. It is of course, a proof, that the lava erupted had not been derived from that source.

2d, The products of volcanoes are totally unlike those which are supposed in the Huttonian theory to be formed and thrown out from the central regions. The latter consist of granite, porphyry, and trap, substances never rejected by volcanoes. On the other hand, sulphur is an abundant production of volcanic fires, while it is never present in the unstratified rocks; a proof of itself decisive that the matter ejected from volcanoes is not derived from the same source with that which is supposed to give rise to the products of the central regions. The stony matter thrown from volcanoes appears indeed, from many facts, to be merely the rocks and fossils

of the country, either fused or partially altered by the volcanic fire.

Lastly; The extinction of volcanoes sufficiently proves that they arise merely from the burning, or mutual chemical action, of a quantity of matter locally accumulated, and spent after a certain period. If they were connected with the central regions, no such extinction should take place.

The evidence from these facts is decisive in proving that the volcanic mountain is the seat of the operations the volcano performs; and the phenomena, therefore, to which these give rise, are no proof whatever of a central fire.

The force of some of the difficulties which have been stated in the course of this discussion, and the impossibility of obviating them, seems to have been perceived by Dr. Hutton himself. In answer to the objections of Mr. Kirwan, he makes the following observations, from which, however he attempts to disguise it, this is sufficiently evident. " I give myself little or no
" trouble about that fire, (the fire necessary to
" fuse minerals) or take no charge with regard
" to the procuring of that power, as I have not
" founded my theory on the *supposition* of sub-
" terraneous fire, however that fire properly fol-
" lows as a conclusion from those appearances on
" which the theory is founded. My theory is
" founded upon the general appearances of mine-

" ral bodies, and upon this, that mineral bodies
 " must necessarily have been in a state of fusion.
 " I do not pretend to prove demonstratively
 " that they had been even hot ; however, that
 " conclusion also naturally follows from their
 " having been in fusion. It is sufficient for
 " me to demonstrate, that these bodies must
 " have been more or less in a state of soft-
 " ness and fluidity without any species of solu-
 " tion. I do not say that this fluidity had been
 " without heat, but if that had been the case,
 " it would have answered equally well the pur-
 " pose of my theory, so far as this went to ex-
 " plain the consolidation of strata or mineral
 " bodies, which I still repeat must have been by
 " simple fluidity, and not by any species of so-
 " lution, or any other solvent than that univer-
 " sal one which permeates all bodies, and which
 " makes them fluid.

" Our author, (Mr. Kirwan), has justly
 " remarked the difficulty of fire burning be-
 " low the earth and sea. It is not my purpose
 " here to endeavour to remove those difficulties,
 " which perhaps only exist in those suppositions
 " which are made on this occasion ; my purpose
 " is to show that he had no immediate concern
 " with that question in discussing the subject of
 " the consolidation which we actually find in
 " the strata of the earth, unless my theory with

“ regard to the igneous origin of stony substances
 “ had proceeded upon the supposition of a sub-
 “ terraneous fire. It is surely one thing to em-
 “ ploy fire and heat to melt mineral bodies, in
 “ supposing this to be the cause of their conso-
 “ lidation, and another thing to acknowledge
 “ fire or heat as having been exerted upon mi-
 “ neral bodies, when it is clearly proved from
 “ actual appearances, that these bodies had been
 “ in a melted state or that of simple fluidity,
 “ These are distinctions which would be thrown
 “ away upon the vulgar, but to a man of science,
 “ who analyses arguments, and reasons strictly
 “ from effect to cause, this is, I believe, the
 “ proper way of coming at the truth *.”

The observations of Professor Playfair are to
 the same purpose : They admit in effect the diffi-
 culties attending the supposition of a central
 heat by which minerals have been fused, and en-
 deavour to lessen these difficulties, on the ground
 that mineral substances are proved from their
 appearance to have already undergone such an
 operation. “ We are not entitled, according
 “ to any rules of philosophical investigation, to
 “ reject a principle to which we are fairly led
 “ by an induction from facts, merely because
 “ we cannot give a satisfactory explanation of it.

* Theory of the Earth, vol. I. p. 237.

" It would be a very unsound view of physical
 " science which would induce one to deny the
 " principle of gravitation, though he cannot
 " explain it, or even though the admission of it
 " reduces him to great metaphysical difficulties.
 " If indeed a downright absurdity, or inconsis-
 " tency with known and established facts, be
 " involved in any principle, it ought not to be
 " admitted, however it may seem calculated to
 " explain other appearances. If, for instance,
 " Dr. Hutton held that combustion was carried
 " on in a region where there was no vital air,
 " we should have said that he admitted an ab-
 " surdity, and that a theory founded on such
 " postulata was worse than chimerical. But if
 " the only thing imputable to him is, that, being
 " led by induction to admit the fusion of mine-
 " ral substances in the bowels of the earth, he
 " has assumed the existence of such heat as was
 " sufficient for this fusion, though he is unable
 " to assign the cause of it, I believe it will be
 " found that his system only shares in an imper-
 " fection which is common to all physical theories,
 " and which the utmost improvement of science
 " will never completely remove*."

These observations are so far just, that the
 merits of any geological theory must rest on its

* Illustrations, &c. p. 189.

according with the appearances of minerals. If it fully agrees with these appearances, the cause, thus established by induction, ought to be admitted, though it may be liable to difficulties as to its mode of production or operation. But in the application of these observations to the Huttonian hypothesis there seems to be a mistake, and the reasoning under which its defenders shelter themselves as a last resource, is in several respects incorrect.

It will be recollected, in the first place, that the term *proof* used in this reasoning, is not to be understood in the sense which strictly belongs to it, but in a more loose signification. When Dr. Hutton says that he has *proved* minerals to have been formed by fusion, he cannot pretend that his proof is of that kind from which corollaries may be strictly deduced, as from a mathematical demonstration. The proof can, from the nature of the subject, amount to no more than a high probability, and in establishing it, therefore, all the circumstances are to be kept in view, and allowed their due weight. If difficulties attend the conclusion, that minerals have been formed by subterraneous heat, these must enter into the calculation of probabilities from which the conclusion itself is to be drawn. If these difficulties be of importance, they may even be capable of balancing probabilities from

the appearances of minerals ; or if, on the other hand, the proof from induction be in any respect doubtful or incomplete, such difficulties may be sufficient to justify the rejection of the principle partly assumed and partly attempted to be proved. We are not allowed, therefore, to draw the conclusion without regarding these difficulties *à priori*, and then say they are of no avail, because the proof is already established.

But, farther, though it were admitted as really proved, that minerals have been formed by fusion, it is not this simple proposition merely which constitutes the Huttonian hypothesis ; it is a detailed system, in which a number of successive operations are supposed. If, in the series, one operation can be pointed out, for the uniform production of which provision is not made, the system is subverted, even though other propositions which it embraces may be fully proved. From the nature of heat, it can be shown that this power cannot be accumulated in the central parts of the globe, to the extent which is necessary in the Huttonian doctrine, and that there is no source from which it can be regularly supplied. Though it were, therefore, proved by indisputable induction from the qualities of minerals, that they have been formed by fusion, this would not prove the truth of the Huttonian hypothesis. The conclusion must mere-

ly be admitted, that they have been formed by fire, but not that the fire has been applied in the mode, and under the circumstances which that hypothesis points out. Were it even proved, from the positions of the strata, that they had been formed and elevated by a subterranean heat, this would still not prove the peculiarities of the system, that these must have originated from a former continent; that the materials arising from its waste are subjected to a central heat which at all times continues to operate, and that a successive elevation of them takes place. In a word, were the igneous origin of fossils clearly demonstrated, it would serve only to establish a Vulcanic theory, but would no more prove the truth of the particular system of Hutton, than it would that of Leibnitz or Buffon. It is to no purpose, therefore, that we are told by Dr. Hutton, that he has proved that minerals have been formed by fusion. This, were it granted, does not prove, nor even tend to establish, the several propositions which constitute his system. Some of these are, on the contrary, proved to be false; and, therefore, were the proof of the first proposition admitted, the refutation of the entire system, or the demonstration of its falsity would still be complete.

Lastly, when the Huttonian doctrine is tried

by the test which Professor Playfair himself has properly pointed out, the decision must be given against it. "If," says he, "a downright absurdity or inconsistency with known and established facts be involved in any principle, it ought not to be admitted, however it may seem calculated to explain other appearances." It is a downright absurdity to suppose that caloric could be propagated from the centre of the earth to the bottom of the sea, so as to consolidate the loose materials there collected, and should never be able to extend itself farther. It is an inconsistency with known and established facts to suppose that caloric may exist in an active state in part of a mass of matter without being propagated through the whole; and the principle, therefore, grounded on this supposition, must be rejected, however perfect its explanation of phenomena might be. The supposition itself is just as absurd as that which the Professor has stated, as sufficient, if it had been maintained, to overturn the theory, that combustion may be carried on where there was no vital air; for we have not more complete evidence establishing the truth as to the one of these points, than we have as to the other. The fact is as clearly demonstrated, that caloric diffuses itself over matter till an equilibrium of tempe-

ture is attained, as that the presence of oxygen is necessary for the process of combustion; and any principle which contradicts the one must be regarded as false, as much as that which contradicts the other. We may, therefore, pronounce in the words of Mr. Playfair, " that a theory founded on such *postulata* is worse than chimerical."

The extensive discussion on this first principle of the Huttonian theory, may demand some apology. Its importance affords one that is sufficient. The evidence for a geological theory, from the appearances of minerals, must often be imperfect or ambiguous, and it evidently is so, since different opinions are held on the subject. But when the first principles of a system are proved to involve absurdities, and contradictions with known facts, the refutation of it is necessarily more complete.

The discussion is of importance likewise in the subsequent investigation. No hypothesis was ever broached which did not explain, and explain even in a satisfactory manner, some of the phenomena it is designed to embrace; and this will no doubt be found to hold true of the Huttonian theory. But when the falsity of its principles is thus demonstrated, these explanations will be regarded in their true light—as skilful or fortunate adaptations of the hypothesis to the

phenomena, and not as just interpretations of nature.

The last general principle of the Huttonian system is, that the strata after having been fused and consolidated by subterranean heat, had been elevated by the same power. To its probability, *à priori*, the same objection may be made which has been already urged with respect to the fusion of the strata,—the difficulty or impossibility of obtaining and preserving a degree of heat sufficient for such a purpose. And were this granted, no principle is pointed out in the theory by which the action of this power can be regulated. It is always represented as the peculiar excellence of this system, that none of its operations are the result of accident, but all are adapted to the attainment of a determinate end. Yet in the supposition of this elevation of the strata by an expansive power, no cause is pointed out for its regular exertion in the mode the theory supposes, or why it should not be occasionally the cause of havoc and disorder, as of the renovation of a continent; and why it might not elevate these strata before they had been fully prepared. For the production of the effect the Huttonian theory ascribes to it, it is necessary that it should be exerted on the strata, consolidated and ready for elevation at the bot-

tom of the sea. Yet there is nothing connected with it, no circumstance pointed out which should preserve it within these limits, or cause it to act there more than upon the elevated land. The principle assumed, therefore, is at once gratuitous and improbable. How far it accounts for the appearances which the strata exhibit, is afterwards to be considered.

We are now to consider the Neptunian theory with regard to the probability of its first principle,—that the different fossils have been formed, and the strata arranged by deposition from water.

The great objection to this principle is, the insolubility in water of the matter of which these strata consist. The simple earths which are found in any considerable quantity in nature, as lime, argil, magnesia, and flint, are very sparingly soluble in it; and the compounds which they form by their union, the different earthy fossils, are many, or even the greater number of them, apparently insoluble. How therefore can it be supposed that water is the agent which has given them fluidity, or that they have been consolidated from aqueous solution? “ To affirm
 “ that water was ever capable of dissolving these
 “ substances, is to ascribe to it powers which it
 “ confessedly has not at present; and therefore

“ it is to introduce an hypothesis, not merely gratuitous, but one which, physically speaking, is absurd and impossible *.”

In strict reasoning, the Neptunist may decline answering objections of this kind. The only principle which he assumes is, that fossils must have been formed by consolidation from aqueous solution, because the appearances they present are incompatible with the supposition of their being formed by fire. If he can establish this, his opinion is proved, and his reasoning may therefore be entirely confined to it. He may reject, as subjects beyond the reach of investigation, the inquiries respecting the manner in which this aqueous solution and the consolidation from it were effected ; and if pressed with objections from these topics, the answer is sufficient, that, in the operations of nature, effects may have arisen from causes, though in the present state of our knowledge we may be unable to point out how from such causes they could have been produced. The Huttonian geologist, by aspiring to higher ground, occupies a less advantageous position. He presents us with a connected system, in which not only a particular cause is pointed out for the production of certain effects, but in which also is detailed how

* Illustrations, p. 18.

and where, with what force, and under what modifying circumstances this cause has operated; and consequently objections to any of these parts of his doctrine it is incumbent on him to answer. But the Neptunist may rest satisfied with the induction from facts, that water has been the chief agent in the formation of minerals, and his conclusion will be just, though he may be unable to point out how it has operated.

Let us endeavour to discover, however, if the force of this great objection to the Neptunian theory may not be obviated.

It may be remarked, that the reasoning from which it derives its force is of that faulty kind which a sound logic proscribes. We reason confidently from what is at present to what has been, and suppose unreasonably that fossils must have been the same at their formation and arrangement as they now are; that they must have been in the same state of aggregation, must have exerted the same affinities, and had the same relations to each other. Yet nothing is more certain than that in these respects they must have been extremely different, and such differences must have produced the most important effects.

Thus it is a principle now admitted by every chemist, that a substance in mass, from its state of aggregation, may be insoluble in a fluid in

which, when in a state of extreme mechanical division, it may be dissolved. The examples of this kind in the mineral kingdom are numerous. The corundum stone, though consisting principally of argillaceous earth, is insoluble in any acid till its cohesion be overcome. Flint is not acted on by an alkali, unless it has previously been reduced to a state of mechanical division. The jargon resists every attempt to decompose it in the humid way, till its aggregation is overcome by the joint action of potash and a strong heat. The native oxyd of tin is from the same cause insoluble in any acid. And in general, in mineral analysis, this insolubility from aggregation, requires to be overcome by various preliminary operations.

It is apparently from this cause that several of the earths and earthy fossils have been considered as incapable of being dissolved by water, though soluble in that fluid. This is particularly exemplified in *filex*, the earth which has been deemed the least soluble of any, and of course with respect to which the Neptunist finds the greatest difficulty. When triturated with water, no sensible portion of it is dissolved; yet there are a variety of facts which prove it to be soluble. If, for example, the *filex* be combined by fusion with an alkali, and if this compound be afterwards decomposed by an acid, the fili-

aceous earth thrown down is in a state of extreme mechanical division, into which it cannot be brought by any other means; and in this state it is soluble in water; so that if a large quantity be employed to dilute it, the earth is not precipitated. Siliceous stalactites, though rare, are sometimes however found, as is admitted by Dr. Hutton himself; and in nature, flint has actually been found dissolved in many mineral waters. It is sufficient to give as an example of this the water of the Geyser fountain in Iceland. A hundred cubic inches of this water were found by Klaproth to hold dissolved not less than nine grains of flint, and by Dr. Black 10.8 grains. The latter illustrious chemist supposed that the solubility of the earth might be promoted by a portion of soda likewise present in the water. But Klaproth has justly observed, that the quantity of this is comparatively so small, (only three grains according to his analysis, and 1.5 according to Dr. Black's, in 100 cubic inches of the water) that it is altogether inadequate to the production of such an effect; and that, besides this, the soda is neutralised by carbonic acid, while it is only the pure alkali that promotes the solubility of this earth. Flint has also been found dissolved in water in which no alkali was present*. No

* Kirwan's Geological Essays, p. 117.

doubt therefore can remain of the justness of Klaproth's opinion, that this earth exists in the water of this fountain, from its natural solubility in that fluid, assisted probably by the high temperature, since we find it deposited at the sides of the fountain; a deposition which must be owing to the temperature of the spring being reduced on exposure to the air.

It is proved, therefore, that siliceous earth is soluble in water, and that its apparent insolubility is owing merely to its state of aggregation. It follows, that the same cause must be assigned for the insolubility of other fossils; and, of course, that, though now insoluble from their state of aggregation, they may still have had an aqueous origin. To those with whom an example is more convincing than reasoning, this will be rendered evident by a simple fact. The siliceous deposition at the Geyser fountain, is composed, according to Klaproth, of 98 of silex, 1.5 of argil, and 0.5 of oxyd of iron; it is frequently as hard as agate, and is insoluble in water, as much as any earthy fossil. Yet it owes its formation to aqueous solution; a fact sufficient to convince us of the error of the conclusion, that fossils cannot be formed by water, because, after their consolidation, we find them not soluble in that fluid. It is, indeed, an actual demonstration, that such a conclusion is false.

Farther, the solvent power of water, with respect to any substance, is invariably promoted by heat. If we conceive, that, at the commencement of the formation of our strata, this fluid held in solution a great quantity of saline, earthy, and metallic matters, it is certain that it would be capable of sustaining a much higher temperature than pure water would; and this high temperature, it is indubitable, would augment its solvent power. It is nearly demonstrated, that, by this agent, the power of combination, of which solution is merely a particular case, may be increased to any requisite extent. The researches of Berthollet on chemical affinity, and a number of facts recently established, have rendered it extremely probable, that there are no two bodies in nature which have not attractions to each other; that water, for example, has not only an attraction to many substances, but to all; and that, in particular cases, existing attractions are prevented from being efficacious, only by the aggregation in the bodies concerned being superior to the chemical attraction exerted between them, or to other circumstances preventing their union. The former power, aggregation, is uniformly diminished by heat; and it follows nearly as a corollary, that, by its action applied in a sufficient degree, any two bodies may be made to

combine, or any fluid may be made the solvent of any solid. Experiment likewise establishes this conclusion; for, when water is made to sustain a high heat, it becomes the solvent of substances, of which, at a low temperature, it appears to be incapable of dissolving even the most minute portion.

Now, it is no improbable supposition, that, at the period when the materials of the surface of our globe were in solution, the temperature may have been much superior to that which is at present necessary for the operations of nature. There must, necessarily, have been assigned to this planet, a certain quantity of caloric, and this, before that order was established which now prevails, may have been locally accumulated, and may thus have been capable of producing, at the surface, the greatest effects. It has also been the opinion of several geologists, (and, as an hypothesis, there is nothing to prevent it from being assumed) that, at this period, the atmosphere was not formed; of course, the immense quantity of latent heat which it now contains, would be sensible and active in the fluid mass; and, from this cause alone, its temperature must have been high. It is needless to repeat, that, from such a cause, the extent of which it is not easy to assign, it is impossible to calculate the effects; and he who would limit

them, who would say that such a power could not be present, or that it was incapable of the operations supposed, would reason in opposition to principles established by indubitable evidence. This assertion would even stand contradicted by facts; for, the solution of flint in the Bath waters, Carlsbad waters, those of the Geyser fountain, and other hot springs, prove the reality of such a power in heat, and that it is capable of producing such effects.

It is now likewise admitted, that it is impossible to determine the force of a chemical attraction between any two bodies, or the effect which would arise from its exertion, otherwise than by observation or experiment. And the proposition, independent of all theory, would at present receive the assent of every chemist, that, if a number of substances were brought into contact, in a state of extreme division, by the medium of a fluid, which itself exerted attractions to many of them, it would be impossible to estimate what attractions would be most efficacious, what would be the result of their concurrent exertion, or what might be supposed to be the effect arising from the presence of any of them.

To speak more precisely with regard to the present question, it is supposed in the Neptunian theory, that at the period of the formation of our

strata, there were dissolved in the chaotic fluid, the different simple earths; the simple inflammables, the metals; with various saline matters not decomposed, or if these are compounds, the elements of which they consist. To these, most probably, are to be added the elastic fluids, which at present form our atmosphere, and all those principles which are now accumulated in the animal and vegetable systems. By what power were these substances held in solution by the water? The Huttonian, in his argument against the Neptunian hypothesis, replies, that it must be supposed to be by the attraction of each of them separately to that fluid. But the reply is absurd. Each substance would exert an attraction more or less efficacious to every other present, or at least, according to the old chemical notion, it would exert attractions to many of them; and the effect which would result from these complicated attractions, it is impossible to estimate. The number of simple substances found in nature, and which must of course have been all present in this fluid, exceeds forty. Suppose the half of these to be in such small quantities as to be imperceptible in their action, still, from the numerous attractions of the others, any imaginable effect might arise.

To illustrate this position, if we take one of

these substances, suppose the oxygen, which now constitutes one fourth of our atmosphere, and which enters into the composition of all the vegetable and animal substances at present existing in such abundance, how are we to determine what may have been the effects resulting from the exertion of its attractions, when deprived of its elasticity? To specify one of them, it is capable of combining with all the metals; and it not only promotes their solubility in water, but increases the force of their attractions to the earths, or the facility of their combination with them. How, therefore, are we to follow it through these numerous combinations, so as to determine the ultimate general effect? Or take hydrogen as an example: it is capable of combining with sulphur, and with carbon; and these compounds again become solvents of other matters. Hydrogen is even capable, in its nascent state, of dissolving some of the metals, and when deprived of its elasticity, or presented in a state of condensation, it is possible it might be capable of forming combinations with the whole of them. Or, lastly, let us take as our illustration the muriat of soda, which exists in such immense quantity in nature: It is evident, that if we suppose its principles, the acid and the alkali compounds, as their simple elements are unknown to us, we introduce the operations of substances, which, so far as we know, may

be fully adequate of themselves to effect the solution of every kind of matter in the chaotic fluid : Or if, as they have not been decomposed, we suppose them simple, we are not to imagine that they would exist in a mere binary combination, or as muriat of soda ; their attractions would be divided and modified by the other substances present, in such a manner, that he would expose himself to the charge of ignorance of chemistry, who would venture to foretell the result. When to these three natural substances which we have taken as examples, the affinities of so many others are added, it is obvious, that, from such complicated attractions, any effect which implies not a physical impossibility might be produced.

Another clear illustration of the same truth is afforded by a singular fact which has been stated, and which on experiment will be found to be just. When the alkaline solutions of filex and argil are mixed together in equal proportions, “ a firm, gelatinous, opalescent mass, results in a very few minutes. This is perfectly insoluble in water, yet soluble in acids, whether concentrated or diluted, nay even in distilled vinegar, and yet consists of both filex and argil : Here, therefore, the properties of the filex must be considerably altered *.”

* Nicolson's Journal, vol. iv. p. 543.

No one could have imagined, *à priori*, that argil could render filex soluble in acids, in which by itself it is perfectly insoluble ; and from this single fact it is obvious, that the conclusion is equally probable, that another substance, by the attraction it exerts to filex, might render it soluble in water. It places, indeed, in a clear light, the influence arising from attractions, which we should not have supposed would be important ; and fully proves, that the effects which may result from the mutual chemical actions of a number of substances, cannot possibly be estimated. It is a fact of the very first importance, and which cannot be too highly prized by the Neptunian geologist.

Reasoning of this kind, though in itself sufficiently conclusive, is always rendered more forcible when it can be shown, that effects, analogous to what are supposed, actually exist in nature, and arise from causes of a similar kind. In the present case we can obtain the advantage of such an illustration. In the formation of the animal and vegetable substances, affinities are exerted of which we have no knowledge, but from the products that are formed, and combinations take place which we could not have imagined, and which we cannot imitate. A few simple substances, carbon, hydrogen, oxygen, azote, and phosphorus, only are

concerned ; but so far from forming the few binary, or ternary compounds which art can produce by combining them, they form an innumerable variety of compounds, distinguished by the possession of the most opposite properties. To what cause is this to be referred ? The physiologist may content himself with ascribing it to the superintending influence of an unknown vital power. But if we are not to satisfy ourselves with a term destitute of meaning, and which, so far from solving the problem, conveys no precise idea, we will search for some more intelligible theory. We will find it in the circumstances in which these substances are placed. We now know that chemical affinity is not to be regarded as an absolute power, which, in all cases where it can operate, will do so with an equal force. It is most materially influenced in its action by a variety of circumstances ; by force of aggregation, distances of the particles, temperature, elasticity or condensation of the agents concerned, quantity of the mass, and others perhaps not ascertained. Wherever these are varied, especially where several substances, having mutual attractions, are present, a difference will take place in the affinities exerted, and thus, from a very slight variation of circumstances, the most varied combinations will result. Art cannot easily pro-

cure or regulate such variations, and she therefore cannot imitate the operations of animated nature, in which, by the most complicated arrangement, the most ample provision is made for the attainment of these modifying powers. Hence, as has been said, effects result on which we could not, *à priori*, calculate; and the inference from this, to these operations by which the materials of our strata were dissolved and consolidated, is direct and incontrovertible. Attractions must have been exerted, of which we know neither the number nor absolute force; and these must have been indefinitely modified, by the circumstances of temperature, quantity, condensation, and others which we cannot determine, and which, in the instance of the organic products now pointed out, produce such important effects. He, therefore, would reason strangely, who would abstract entirely these circumstances, and tell us, that because when flint is triturated in a mortar with water it is not dissolved, it is a physical impossibility that siliceous earth should have been in solution in the chaotic fluid. His reasoning would be nearly the same as that by which it should be affirmed, that sugar cannot be a compound of carbon, hydrogen, and oxygen, because we cannot form it by combining these principles, or

are unable, even by any means, to bring them into a ternary combination.

Lastly, we do not know what really are the simple principles of the substances existing in the mineral kingdom ; and this of itself is sufficient to solve the whole difficulty. If we analyse an earthy fossil, we find our analysis cannot be carried beyond certain substances, which we term simple earths : or if we examine the composition of an ore, we resolve it into a metal combined with some other body. But can we affirm that these earths and metals are certainly simple bodies, or that they may not be compounds ? He who knows any thing of the principles of chemistry, or even of its history, would never consider the former conclusion as established, but would rather incline to the latter, since our analysis can never prove a body to be absolutely simple ; and the progress of discovery has invariably been, to prove substances, apparently so, to be compounds, and compounds too of a nature very different from what the state of knowledge prior to the discovery would have suggested. If some years back an opinion on any subject had been maintained which required the supposition that water and air were compounds, it would probably, from the prevalent notion of these bodies being elements, have been rejected, yet the event has shown

that it would have been just. And on the same grounds, it is equally allowable at present to suppose, if it be necessary to do so to solve a difficulty attending a principle established on evidence, that the earths and metals may be compounds ; and indeed in itself such a supposition is entitled to equal regard with that which considers them as simple, and is perhaps more conformable to truth. As a general rule to guard against extravagant speculation, or as a principle which regulates our arrangements, it may not be improper to rank every substance as simple which is not decomposed ; but it should not be forgotten, that this is a mere supposition, admitted for these reasons, that it is only a principle of convenience, but that in the abstract it is not more certain than the opposite conclusion.

We should not indeed err much, perhaps, if we considered the greater number of bodies which are at present the subjects of our knowledge as compounds. Chemistry is but in its infancy ; within a few years only has it discovered the composition of a number of substances ; and shall we believe that it has already attained the end of its researches, and that the varieties of matter which analysis has discovered are truly elementary ? Bodies, as they approach to simplicity, apparently become more subtle,

as is evident from contrasting the simple gases with the compounds they form. When we compare the amazing subtilty and tenuity of light, and those kinds of matter which give rise to the phenomena of magnetism, electricity, and galvanism, with the grossness and sluggishness of the metals and earths, or even the simple gases, we will perceive that here the usual gradations of nature are not observed; that the chain which should connect material bodies is as it were broken between these different classes; and that the supposition is not improbable that these grosser bodies are compounds of others more subtle, which may approach, or graduate into those kinds of matter that have the most undoubted claim to the character of simplicity. There are likewise a variety of facts both in chemistry and mineralogy, such as the apparent transmutation of flint into calcareous earth, and the production of the earths in the vegetable system, which appear to indicate that these are compounds.

To such suppositions, however, though in themselves sufficiently probable, and not unsupported, we need not have recourse. It is sufficient that the absolute simplicity of these bodies,—the earths and metals, is not proved. If the possibility of their being compounds be admitted, (and its impossibility the Huttonian

will never be able to demonstrate) the objection to the Neptunian theory of their formation, that they are nearly insoluble in water, is of no force. It is obvious, that if compounds, their principles might exist in the fluid or even aerial form, that they might be soluble in water, or by their mutual attraction might contribute reciprocally to their solution, and that when combined in other modes they might form those less soluble compounds which now exist.

When we consider the circumstances now enumerated, the influence of aggregation in preventing solution, the power of temperature in promoting it, the incalculable effects resulting from the exertion of complicated affinities, and the possibility of substances being compounds, which our imperfect knowledge ranks as simple, we can have no hesitation in admitting the conclusion which each separately establishes, that fossils may have been formed by water, though apparently insoluble in that fluid. And if an induction from facts shall render probable their aqueous origin, their present insolubility will form no objection of real force.

Dr. Hutton has likewise supposed, that, granting the solubility in water of the matter of which our strata consist, their subsequent consolidation cannot be accounted for on the Neptunian theory. The porosity of the mass could never be

" For the bulk of the solvent, and of the mat-
 " ter it contained in solution, being greater
 " than the bulk of either taken singly, when
 " the latter was deposited, the former would
 " have sufficient room left, and would continue
 " to occupy a certain space in the interior of
 " the strata. A liquid solvent, therefore, could
 " never shut up the pores of a body, to the en-
 " tire exclusion of itself; and, had mineral sub-
 " stances been consolidated, as here supposed,
 " the solvent ought either to remain within
 " them in a liquid state, or, if evaporated, should
 " have left the pores empty, and the body per-
 " vious to water. Neither of these, however,
 " is the fact; many stratified bodies are per-
 " fectly impervious to water, and few mineral
 " substances contain water in a liquid state.
 " That they sometimes contain it, chemically
 " united to them, is no proof of their solidity
 " having been brought about by that fluid;
 " for such chemical union is as consistent with
 " the supposition of igneous as of aqueous con-
 " solidation, since the region in which the fire
 " was applied, on every hypothesis, must have
 " abounded with humidity *."

These objections, it may be affirmed, arise
 from ascribing an hypothesis to the Neptunian

* Illustrations of the Huttonian Theory, p. 16, 17.

theory which does not belong to it, and that, with respect to this point, it labours under no real difficulty. This will be evident, from stating briefly the modes in which it supposes consolidation to take place from fluidity by water.

If the solid substance be completely dissolved in the water, it may separate from it by crystallization, this crystallization being determined by a change of circumstances in the solution. If the solubility of the solid had been augmented by the heat of the solvent, or by the presence in it of certain principles, suppose of an aerial kind, it is evident, that on a reduction of temperature, or on the escape of these principles, or even on new combinations taking place, crystallization would commence: it would proceed more or less rapidly according to the extent in the change of circumstances bringing it about; and in this manner might be formed the different strata which exhibit marks of crystallization in their structure. No one can doubt that in this way masses of the greatest hardness and density might be formed, and the chemist indeed has daily opportunities of observing the hardness which a crystalline mass acquires, when it stands for some time with the liquor over it from which it had been deposited, every vacuity which may have been left in it at its first crystallization being filled up by subse-

quent crystalline deposition, and the fluid filling these cavities, being extruded.

Other strata may have been formed merely by deposition of solid matter, not dissolved, but suspended in a fluid. In this manner have apparently originated a considerable number of the secondary strata. The particles, deposited in large beds, would at first have little cohesion; but there is no improbability in the supposition, that in this soft state the attraction of aggregation would be exerted between them, since we know that neither this species of attraction, nor chemical affinity, is entirely confined in its action to the ultimate particles of matter, but can be exerted between minute masses. The particles would thus approximate, the interposed fluid would be forced out, and a mass more or less compact would be formed. This, on remaining a long time at rest, might attain a great degree of hardness; and it may be observed, that in nature no fossil, at least none uncrystallized, is so perfectly dense as to be altogether impervious to water. This species of consolidation would be still more promoted if a chemical were mixed with the mechanical deposit, or matter was separated that had been dissolved, at the same time with the subsidence of the particles suspended, a circumstance which appears to have often happened.

The futility of Dr. Hutton's objections to this account of consolidation from aqueous solution will now be apparent. They do not indeed apply to it, but to an hypothesis which no Neptunist maintains. In accounting for the consolidation of the strata by water, it is not conceived that the loose materials are first deposited at the bottom of the sea, and that then some matter is introduced into their pores by which they are rendered hard. They have been formed either by crystallization or precipitation. In the former mode it is obvious that no operation of this kind can be supposed requisite to consolidate them; in the latter case it is in general equally unnecessary, because when the particles in a state of minute division were deposited, they might cohere, and would not, as Dr. Hutton conceives, require the introduction of any other matter to complete their consolidation. But were even such a supposition made, the objection would be groundless. If the water supposed to be introduced into the precipitated mass had particles of solid matter merely *suspended* in it, these would be disposed to subside, and cohere more or less firmly with the solid matter into which the water had been insinuated, and the water itself would at the same time be extruded. Or if the solid matter were *dissolved*, it would be disposed to crystallize, by obtaining a nucleus

The cause of this singularly confused and mistaken statement, appears to be, Dr. Hutton having always before him a limited view of his subject. When he asks, by what cause are the strata consolidated? he appears to mean, not as his expression would imply, the strata in general, but those particular strata which consist of mechanical fragments cemented together. He considers these strata as composed of particular collections of substances, limestone, for example, of remains of marine animals, and pudding-stone, of pebbles or gravel, which are consolidated by being fixed in a common cement. When, therefore, his question is answered, by saying, that consolidation is owing to the mutual attraction of the particles, the answer appears to him preposterous. But this is owing to the preposterous manner in which he has conceived the subject. It is a proper answer with respect to the consolidation of homogenous strata, which amount at least to nine in ten of the whole. And, if the question were farther put, with respect to these particular strata, which are composed of mechanical fragments cemented together, the answer of the Neptunian would then be, that the cementing matter of these strata had been deposited around these fragments from solution or suspension in water, and that the attraction of aggregation being exerted

between its particles, it had consolidated, and, of course, had consolidated the whole mass.

Or rather the limited view which Dr. Hutton takes of this subject, arises from his hypothesis, that all the strata have arisen from the decay of former ones; and that, therefore, their materials have once consisted of mechanical fragments loosely deposited. He should have recollected, however, that this supposition is not admitted by his antagonist, and that he therefore was not at liberty to attribute it to him, or to reason from it as if it were received. The opinion of the Neptunist as to the original condition of the materials of the strata is altogether different, and it is not to this opinion, but to an hypothesis, the creature of his own imagination, and of his limited views, that the objections of Dr. Hutton on consolidation apply.

Examples of both the kinds of consolidation pointed out, are abundant in nature. Calcareous crystals and stalactites, and even siliceous stalactites, are frequently formed by water at the surface, and show, at least, that from crystallization, perfect consolidation may take place; a conclusion, indeed, which no one can doubt. Of deposition, too hasty to admit of crystallization, the siliceous incrustation at the Geyser fountain is a sufficient example. Its hardness is frequently such as to be equal to that of the

being consolidated by concretion from water ; nor has it been shown that they ought to contain more water than they actually do. In all crystallized bodies, the quantity retained is different, and is dependent partly on their attraction to the fluid, and partly on circumstances attending their crystallization. And there are substances actually deposited from water which do not contain any sensible portion of it : Such, according to the analysis of Klaproth, is the siliceous incrustation of the Geyser fountain.

We may now contrast these two systems with respect to the probability of their first principles, and compare the explanation which either affords, with that which the nature of the subject admits. We have seen, not only that the Huttonian system labours under insurmountable difficulties, in accounting for the production of that heat by which it supposes the strata to be fused and consolidated, but it has been proved, that its principles stand in direct opposition to established facts, and are therefore false. The Neptunian system, again, in accounting for the original fluidity of the surface of the globe from the operation of water, may appear to be liable to some difficulties ; but it is fertile in resources, since causes can be pointed out, by which the

action of that fluid might be modified, so as to produce the effects ascribed to its operation. It may be impossible to determine which of the causes pointed out did actually operate, or to which the greatest share in the ultimate effect is to be attributed. But what is to be expected from the geologist in such researches? not surely that he is to point out the precise manner in which these immense operations have been conducted, or, as if he had been a spectator of them, describe them in detail. The theorist may make pretensions of this kind, but, under the name of a system, he will give us merely a dream. It is sufficient if, from induction, a particular cause be established, and if this cause be rendered probable, or objections to it obviated by reasoning which involves no improbable suppositions. This is attained in the Neptunian theory. From the appearances of minerals, we shall find sufficient reason to conclude, that they have been formed by water; and the objections which might, *a priori*, be made to this conclusion, from the powers of water being inadequate to such effects, we have seen are sufficiently obviated, by the admission of certain modifying circumstances, which not only might have operated, but which must have done so to a certain extent. There is, therefore, nothing to

prevent the admission of the principle which induction establishes.

On this subject too, were it necessary, the principle might be insisted on, that, if a certain cause be suggested by the phenomena, it ought to be admitted, though there are difficulties in explaining its precise mode of operation. Dr. Hutton and Professor Playfair apply this argument to the support of their doctrine, without perceiving or acknowledging that it can be brought with equal force to establish the Neptunian theory: "I have proved," says Dr. Hutton, "that those stony substances have been in the fluid state of fusion; and from this I have inferred the former existence of an internal heat, a subterraneous fire, or a certain cause of fusion, by whatever name it shall be called, and by whatever means it shall have been procured. Though I should confess my ignorance with regard to the means of producing fire, the evidence of the melting operation, or former fluidity of these mineral bodies, would not be thereby in the least diminished *." It is obvious, that the whole of this argument may be adapted with equal justice to the Neptunian theory. I have proved, (a defender of that system might say) that these

* Theory of the Earth, vol. i. p. 239.

stony substances have been in the fluid state of solution ; and from this I have inferred the existence of a solvent, or a certain cause of solution, by whatever name it shall be called, and by whatever means it shall have been procured ; and though I should confess my ignorance with regard to the means of procuring this solvent, the evidence of the dissolving operation, or former fluidity of these mineral bodies, would not be thereby in the least diminished.

This mode of reasoning can indeed be employed with much more force and justice by the Neptunist than by the Huttonian ; for, as has already been shown, though it were demonstrated that minerals had been formed by fusion, this would not establish the truth of the Huttonian system, because that system involves principles physically impossible, and which therefore no induction can establish ; while on the contrary, the Neptunian is at most liable to some difficulties which are easily removed, and which, though they were not, would not be sufficient to invalidate the evidence which the appearances of minerals afford. Little doubt, therefore, it is presumed, can remain of the decided superiority of the first principles of the one to those of the other. We have next to examine what support these theories derive from the phenomena of geology. The arguments of this kind which

have been advanced may be considered under two classes; those drawn from the positions and connections of the strata of the earth, and these from the properties and appearances of individual fossils.

PART III,

*Of the Arguments in support of the Huttonian and
Neptunian Theories, from the Positions of the
Strata of the Globe.*

THE immense masses which compose the surface of the earth are extremely various in their positions. Some are altogether irregular, others are disposed in beds or strata which are vertical, horizontal, or inclined at different angles to the horizon, varying in their extent and thickness, alternating often with each other, and frequently intimately connected at their junctions. These are facts of the first importance in geology, and which require to be explained in any theory of the earth.

The Huttonian explanations of the formation of the stratified rocks, and of that of the unstratified, are very different from each other, and require separate consideration.

Of the STRATIFIED ROCKS it has just been observed, that some are vertical in their position, some horizontal, or nearly so, and others inclined at different angles to the horizon. Dr. Hutton supposes that all these have originally been horizontal, that the matter of which they consist has been spread out on the bottom of the ocean, consolidated by fusion by subterraneous heat, and afterwards elevated by the expansive power of that heat, so as to assume the positions they are found to have.

In the Neptunian system it is supposed that the positions of the strata have been determined, partly by the figure of the base or ground on which they have been deposited, and partly by their deposition having been a species of crystallization. The primitive strata, which are generally vertical or highly inclined, had first been deposited from the original fluid in which their substance was dissolved, and they had taken the form in which we find them, by having been hastily crystallized. From these immense masses the water is supposed to have retired into the more hollow parts of the globe, or into caverns in its external parts. This retreat was so gradual as to have continued for several ages, and during its continuance, the secondary strata appear to have been formed by deposition of matter from the ocean,

on the sides of the vertical and highly inclined strata already formed: and lastly, by the continued retreat of the sea, these secondary strata have likewise been left exposed.

It is objected to this account of the formation of the strata, that it does not account for their inclined or vertical position. If matter, it is said, was deposited from a fluid, it would arrange itself in a horizontal bed; the inclination of the bottom or ground on which it is deposited might have some effect in modifying the position, but this would soon cease; the matter precipitated, from this inclination, must be deposited unequally; being carried forward where the inclination is greatest, it would tend to arrange the whole horizontally, and the different beds or strata, as they are formed, would approach more and more to that position. To what cause then are to be ascribed the vertical, or the highly inclined positions which they frequently have, and the parallelism which they preserve with great exactness under very extensive and varied incurvations?

The force of this objection depends entirely on the supposition, that in the deposition of these strata, their matter had been previously merely suspended mechanically in a fluid, and had subsided by rest. Had this been the case they must have been arranged in horizontal beds, and ought

of course to be found in that position. The opinion maintained in the Neptunian theory however is, that they had been chemically dissolved, and had separated and concreted by a species of crystallization. These crystalline deposits would be in large irregular masses, as granite, the rock of primary formation, is ; and the fluid still continuing to deposit matter by crystallization, this matter, in conformity to the laws of that process, would crystallize on the sides of the masses already produced ; and thus the appearance of vertical strata would be formed : or the division of these might even be determined by the process of crystallization itself. With regard to the sinuosities and incurvations in these strata, it may be difficult to point out precisely how crystallization should produce them. But we have sufficient evidence from facts to prove that this process is capable of producing such effects, and actually does produce them in cases so obvious that there is no room for doubt. Saussure has observed, with a view to the illustration of this opinion, which he maintains, that these very appearances are found in stalactites and alabaster. These consist of different layers, which, instead of being in a straight line, are found with all these varieties of bending and waving which in the strata are conspicuous on a large scale. For the production of these no just cause

can be assigned but the crystallization, imperfect as it is, by which these substances are formed, a cause by which not only the figure, as Dr. Hutton has alleged, but likewise the structure of bodies, is determined. It is obvious, that since this is proved to be a sufficient cause, the reasoning is nugatory which is founded merely on the difficulty of conceiving *a priori* how it should produce those effects, because the answer is sufficient that it actually does so. And although we may be unable to say how crystallization should give rise to "the inflexion of the strata, the simple curvature which they affect, and that parallelism of their layers which in all their bendings is so accurately preserved*," yet we see in the example quoted by Saussure that it can produce all these appearances, and therefore it may, on a larger scale, be adequate to their production in the mountains of the globe. The difference of magnitude cannot alter the nature or powers of the process employed.

In reality, some cause of this kind must even be had recourse to by the Huttonian geologist. It is supposed indeed that the stratification has been the effect of deposition from water; but this cannot be admitted; for although the materials of these strata might at their deposition be

* Illustrations, p. 232.

Or if it should even be supposed, that the positions of the strata indicate the exertion of an expansive power from beneath, by which they have been elevated, such a cause, it may be remarked, may be admitted into the Neptunian system as not inconsistent with its principles. The general appearance of these strata prove, that, at the period of their formation, they had been subject to the most important revolutions. They have been disjoined from each other, the beds of rivers hollowed out, islands separated from the mainland; and, in a word, have suffered every species of fracture and dislocation. No cause can be assigned for these effects, more probable perhaps than that of an expansion produced from an accumulation of heat in the interior parts of the globe, the power of which has probably been spent in their production. No appearances in the elevation of these strata prove the peculiar tenets of the Huttonian theory, that the strata thus elevated had once been fused, or that a subterraneous heat has always acted, and still continues undiminished. On the contrary, it has been demonstrated, that these propositions are false, as being utterly inconsistent with the known laws of heat; and it will immediately be shown, that, from such a cause, the original horizontality of these strata cannot possibly be explained. Were it there-

Sandstone is found in beds or strata of very different degrees of thickness, generally horizontal in their position, or somewhat inclined. These are supposed by the Huttonian geologist to have been formed from matter deposited from the ocean, and softened by subterraneous heat. But let him explain by what cause in their subsequent consolidation they have been formed into distinct strata. It may perhaps be said to arise from the contraction of the mass in cooling; and this appears to be the solution which the author of the theory gives: "The contraction of the mass consolidated by fusion, or the effect of fire, is the cause of those natural divisions in the strata*." But why, it may be asked, are the rents horizontal? the direction of all others the most unfavourable, from the gravity of the mass; and why do they preserve this direction with so much regularity to so great an extent? These circumstances clearly prove that the parallelism of these strata is that which they originally had from their deposition from water. Between the different strata there are also often interposed thin layers of earth or clay, a proof that they had been formed successively, and that on each of them, subsequent to its formation, this earthy matter had been

* Theory of the Earth, vol. ii. p. 56.

deposited ; that the whole had not been reduced into a soft or liquid mass, which on cooling had split into horizontal beds, but that the original stratification had been preserved.

This is even admitted by Professor Playfair :
 " In beds of sandstone nothing is more frequent
 " than to see the thin layers of sand separated
 " from one another by layers still finer of coal-
 " ly or micaceous matter, that are almost ex-
 " actly parallel, and continue so to a great ex-
 " tent, without any sensible diminution. These
 " planes can have acquired their parallelism on-
 " ly in consequence of the property of water,
 " by which it renders the surfaces of the layers
 " which it deposits, parallel to its own surface,
 " and therefore parallel to one another *." But
 if this be admitted, the difficulty occurs in its original force. If the sandstone has been consolidated by fusion subsequent to its arrangement, how have these divisions remained distinct, and preserved their exact parallelism ? and how has the interposed clay escaped consolidation ?

Precisely the same difficulty occurs in the primitive strata, which, though now vertical, have, according to the Huttonian geologist, been originally horizontal. No particular fact could be selected more forcible in illustrating this dif-

* Illustrations, &c. p. 44.

ficulty, than one stated by Mr. Playfair: " In
 " the micaceous and talcose shisti, thin layers
 " of sand are often found interposed between
 " the layers of mica or talc. I have a specimen
 " from the summit of one of the highest of the
 " Grampian mountains, where the thin plates
 " of a talcy or asbestine substance are separated
 " by layers of a very fine quartzzy sand not
 " much consolidated *." These kinds of shisti
 having more or less of a crystalline texture,
 must have been nearly, if not completely, fused,
 as is indeed allowed, and even supposed. How
 then have they remained divided into thin lay-
 ers, the parallelism of which is preserved with
 more accuracy, and to a greater extent, than
 even in the secondary strata? Or how, has the
 sand, interposed between these layers, escaped
 perfect consolidation? If it were equally fusi-
 ble with the shistus, or nearly so, it is obvious
 that it ought to have been vitrified or aglutinat-
 ed, and of course perfectly consolidated. If it
 were less fusible, it is not less evident that it
 ought to have been imbedded or cemented in
 the fused shistus. These strata are supposed to
 have been originally horizontal. Suppose the
 first or undermost stratum consolidated, over this
 has been deposited a layer of sand; the forma-

* Illustrations, &c. p. 167.

tion of another stratum of shists by fusion, took place above this. But it is evident that the matter of this stratum, fused or softened, must by its mere weight have enveloped the particles of sand beneath it; and when we reflect on the succession of these layers, the difficulty is much increased: the heat exerted on the undermost layers must have been intense, and the pressure upon these at the same time great. On no hypothesis, therefore, agreeable to the Huttonian theory, could sand, loose or little consolidated, be found between them, nor can the division of these layers themselves be explained.

If we consider this difficulty with regard to strata of different kinds alternating with each other, the argument will be found to gain, if possible, in force. By what cause have the divisions between these different strata been produced? The supposition of successive formation, which in the Neptunian theory fully accounts for such an arrangement, can in the Huttonian have no place. Let us suppose matter to be deposited, fused or softened, and consolidated, how is another stratum of a different kind of matter to be formed above it? Materials for it might be collected, but these, according to the theory, cannot be consolidated without heat. The heat necessary for this purpose being applied from beneath, must operate with still more force on the

stratum on which these loose materials lie, and in innumerable cases must completely fuse it, since in the various positions and alternations of the strata, the less fusible are often placed over those which are more easily fused. There is no provision therefore made in this system for the formation of a number of strata superincumbent on each other. The upper stratum cannot have been last formed, because the heat necessary for its consolidation must have fused these beneath, and the materials of it would of course sink, and be imbedded in the fused matter, before they could be consolidated: The under stratum cannot possibly have been formed, and raised so as to be applied precisely to the one above it, and this through an extensive series: And lastly, the formation cannot be supposed to be simultaneous, for no cause can be given why by the necessary degree of heat the whole should not have been homologated, or how they should afterwards have separated into distinct horizontal strata of the greatest extent, of different materials, and of various degrees of fusibility and states of induration.

In examining the actual positions and connections of the strata, these difficulties might be abundantly illustrated; but to avoid a tedious discussion, it may be sufficient to notice only a

few of these in which they are placed in the clearest light.

No alternation is more frequent than that of calcareous and argillaceous strata. To account for their consolidation, the Huttonian supposes them to have been in a fused or softened state; but if they ever were in such a state, they must, from the attraction between the earths composing them, have combined, and an entire homogeneous mass, not a number of distinct alternating strata, must have been formed; or at least where they were contiguous, they must have united, and every trace of the original stratification have been obliterated: yet so far from there being any union of this kind, the line of separation is perfectly distinct, frequently so much so that they are not in firm adhesion.

Another alternation extremely common is that of limestone with argillite, the alternation being continued through an extensive series. One of these must be less fusible than the other; and as they alternate, it is a matter of perfect indifference in the argument which of them is supposed to be so. Let it be supposed, as is probably the fact, that the limestone is the least fusible: A stratum of it may be considered as commencing the series: on it the materials of the argillite have been deposited from the ocean, and as these are supposed to be more fusible

than the limestone, it is possible that the central heat may have operated through the latter, without fusing it, and have consolidated the former. But above this stratum of argillite is another stratum of limestone. How could it have been consolidated? By the supposition made, the argillite is more fusible; the central heat could not therefore operate through it, so as to consolidate the materials of the superior limestone without fusing it, and from this fusion the materials of the limestone ought necessarily to have been imbedded in the argillite. And when we consider that an alternation of this kind is often found in an extensive series, by which both an intense heat and a great pressure must have been exerted on the lower parts, it is obviously impossible that these different strata could have been consolidated by a central heat, so as to be kept distinct.

Strata of rock-salt are sometimes covered by strata of sandstone, or limestone. The Huttonian geologist must suppose that this sandstone has been consolidated by the central heat acting through the rock-salt below it. But this is plainly an impossibility. The salt is a substance comparatively very fusible, as it can even be volatilized by the heat of a coarse pottery furnace, while sandstone is very infusible. The heat necessary therefore to soften sandstone, in this

position, must have melted the salt beneath ; and as this latter substance is of a much inferior specific gravity, the sandstone must have sunk in it, and the arrangement observed in nature could never have been produced.

Lastly, we find in innumerable cases strata more imperfectly consolidated than others above them, and of course farther removed from the consolidating power, though the difference cannot be ascribed to any difference in the fusibility of the substances composing them. An example will place this in a clear light. In a section of the strata at Newcastle, coal is found at the depth of 102 feet. Over it is a bed of black clay 13 feet thick, with impressions of ferns in its substance ; above this, another bed of harder clay 26 feet in thickness. The stratum incumbent on this is a hard quartzose sandstone, with specks of mica, 25 feet thick ; and this is again covered by clay. Now how could this sandstone have been consolidated by the subterranean heat, while so many feet of clay beneath it, and of course nearer the operation of that heat, had not even been indurated ? We may pronounce it impossible that it should be so. Nor is the example uncommon : there are many similar to it, and even less favourable, as the banks of clay extend to 80, 100, or more fathoms, in thickness, with perfectly consolidated sandstone above ;

and this is diversified with alternations of limestone, gypsum, coal, and a great variety of other secondary strata. It is not possible to conceive an arrangement which more clearly indicates their origin, that these are successive depositions from water, varying in their consolidation, from the different forces of aggregation exerted between the particles of each; and that they have suffered no other change to subvert or modify their original stratification. Could the Huttonian geologists bring forward facts so forcible against the Neptunian theory, it might justify the triumphant tone in which they have sometimes conducted the controversy.

The truth is, that the Huttonian theory has in this part tacitly assumed the explanation of the Neptunian system,—that stratification is the effect of successive deposition from water. Nor has it been observed, that the assumption is obviously incompatible with its principles; since by the subsequent fusion, the original arrangement must have been destroyed. The actual positions of the strata are also totally irreconcilable with the notion of their consolidation having been effected by fusion, as is evident from the instances pointed out; the Huttonian system therefore has the singular infelicity of failing to account for the first and most important of all the facts which are the subject of a theory of the

earth,—the general stratification of the materials at the surface of the globe.

It remains to consider the explanation given in the Huttonian system, of the appearances of the UNSTRATIFIED ROCKS. These are principally granite, and the different varieties of porphyry, basalt, trap or whin, fossils, which, besides their common character of in general not being stratified, graduate into each other.

It has usually been believed by geologists, that the principal rocks of this class are the most ancient in the globe. Granite, in particular, is considered as the base on which all others rest; and therefore as of prior formation to the others. Dr. Hutton holds precisely the opposite opinion, that these rocks are of latest formation, or posterior to the strata, whether primary or secondary, which are incumbent on them.

The formation of these rocks, in the Huttonian system, is ascribed to the eruption of melted matter from the internal parts of the earth, among the superincumbent strata. It is conceived, that, by the subterraneous heat, which is always active, an immense quantity of matter is kept in a melted state; that where the expansive power of this heat is increased to a certain point, the superincumbent strata are heaved up, and a portion of this melted matter ejected; and that

this, when consolidated by cooling, forms granite, porphyry, basalt, or trap, according to the composition of the matter which has been in fusion.

The arguments for the igneous origin of these rocks, from their properties, are afterwards to be considered : At present we are to notice those drawn from their positions.

The principal one of this kind, and on which much is said by the defenders of the Huttonian theory, is that of veins of these unstratified rocks, at least of whin and granite, running into the neighbouring strata. It is admitted, that veins are posterior in formation to the strata in which they are found ; and this appearance, therefore, of veins of whin and granite entering these strata, prove, that the former have been last formed. It is supposed, that the matter of these had been injected, when in a fluid state, into rents and cavities of these strata, and when consolidated, had given rise to these appearances. And, as these veins often proceed from immense masses of whin or granite, or are connected with them, it seems to follow, that these must have had the same origin.

These phenomena are explained by the Neptunist, from the fact, that granite is of different formations. In the Wernerian theory, granite is considered as the rock to which the title of

Primitive most strictly belongs, as being prior in formation to gneiss, micaceous schist, and others of the same class. But, although its positions with regard to these strata generally indicate it to be so, yet in particular instances we are compelled to admit that it has been of later formation. Thus it is sometimes found, that a stratum of granite is incumbent on these other rocks, or alternates with them, which proves that it must have been posterior to them in formation. Specimens have also been found of granite, in which are contained pieces of gneiss, and even argillite, a demonstration of the same fact. This diversity in the times of formation of these rocks holds indeed with respect to them all. When it is said that granite is older than gneiss, and gneiss than micaceous schist, it is not meant that it is universally so, but only generally, and not without exceptions, in each order of these rocks. This being established, the phenomena of granite veins penetrating the other strata, are easily explained; it belongs to the class of facts now stated, which prove, that, in some cases, granite is of later formation than the other rocks named Primitive. According to the Wernerian theory, veins were originally fissures in rocks or strata, which happened while these were still covered with water holding certain matters in solution, and were

filled by these matters being deposited or crystallized. It is conceived, therefore, that, in this manner, granite may have been introduced into fissures of the primitive strata.

Professor Playfair has observed, that this distinction between granite of early, and that of recent formation, is purely hypothetical. "It is," says he, "a fiction, contrived on purpose to reconcile the fact here mentioned with the general system of aqueous deposition, and has no support from any other phenomena *." From the facts above stated, it will be obvious, however, that it is rather a direct inference from phenomena; and the reasoning by which it is deduced is strictly analogous to that by which other geological conclusions are established. In all veins there are observed undoubted marks of different dates of formation, both with respect to each other and to the strata; and there are no grounds for supposing that granite veins are an exception. The assumption, that the granite in mass, and that in veins, are the productions of different periods, is supported by other facts than those from which it is deduced; thus, it is observed, that in many cases, the granite in veins is different in its properties from the other. Nay, this point may be determined by an appeal to the authority of Mr Playfair himself. He admits, that whinstone, into which granite

* Illustrations, &c. p. 320.

graduates, and which belongs to the same class of rocks, is of different formations: " These un-
 " stratified rocks, distinguished by the name of
 " whinstone, are not all the work of the same
 " period; they differ evidently in the date of
 " their formation; and it is not unusual to find
 " tabular masses of one species of whin inter-
 " sected by another *." Now, as this is admitted
 with respect to whin, the probability of its be-
 ing the same with respect to granite can scarce-
 ly be denied, since, in the Huttonian system,
 these two species of rocks are believed to be
 formed in precisely the same manner. Not
 only is there this presumption, but the very
 fact which is stated by Professor Playfair, as
 proving the different formations of whin, is true
 with respect also to granite. It has often been
 observed, that strata of granite are intersected
 by veins likewise of granite, which are distin-
 guishable, however, from the rock in which
 they run, by being of a different colour or grain.
 All veins are admitted, by every mineralogist,
 to be posterior in formation to the strata through
 which they pass; and, from Mr. Playfair's own
 reasoning, which, in this case, is perfectly just,
 this fact must be admitted as decisive proof, that
 these granites are not the production of the
 same period.

Granite, therefore, it may be considered as

* Illustrations, p. 81.

proved, is of different formations ; and the admission of this proposition affords a solution of the phenomenon of granite veins, to which there is no objection.

The same solution may be given of the veins of whin found in the strata ; as it is admitted to have been formed at different periods, it is evident that these veins may be accounted for from its being of recent formation.

It has been stated as an argument in favour of the igneous origin of these rocks, that where they come in contact with the strata, or where veins of them penetrate these strata, the latter are indurated at the point of contact. This is observed particularly in whin, and it is considered as a proof of the whin having been introduced in a melted state, and having, by its heat, consolidated more perfectly the matter of these strata. “ Whether sandy or argillaceous, they “ are usually extremely hard and consolidated ; “ the former, in particular, lose their granulated “ texture, and are sometimes converted into “ perfect jasper *.”

These, and other alterations which the strata suffer, are ascribed, by Werner, to the action of the solvent filling the vein. This will percolate to a certain extent through the neighbouring rock, and, by acting chemically upon it, as

* Illustrations, &c. p. 73.

Werner supposes, or perhaps by depositing part of the matter it has in solution, may alter its appearance and properties. Thus, in the present example, if, into a fissure in a stratum of sandstone, the fluid, holding the matter of trap in solution, were introduced, it would percolate to a greater or less extent through the sandstone, which is generally porous, and the matter deposited in the sandstone by this percolation might give it the hardness of jasper. Jasper is precisely such a fossil as might be expected to originate from an admixture of this kind, as it consists of siliceous, with a large proportion of argillaceous, earth.

This explanation, as an hypothesis, is sufficiently probable, and the great proof that it is just, is, that these alterations in the neighbouring rock are frequently such as the Huttonian theorist cannot explain, on the supposition of the matter of the vein having been in a fused state, and having acted by its heat. They are not always induration, but often a change precisely the reverse. Thus, when veins run through granite or gneiss, the felspar is often changed into kaolin or fine clay, as it is by exposure to air and moisture; the mica is also decomposed, and in sienite the hornblende, so that in the latter a green friable earthy matter is produced; " The mountain of Scharfenberg is of red gra-

“ nite, very hard, and of an equal grain ; it is
 “ traversed by metallic veins, of which the *gan-*
 “ *gue* is white or reddish quartz, calcareous spar,
 “ and white clay. The granite is commonly
 “ much decomposed along the sides of the veins ;
 “ neither its feldspar nor mica are to be found ;
 “ these two substances are supplied by a grey
 “ clay, and a green earth, of the nature of stea-
 “ tite : this decomposition extends ten or
 “ twelve inches on each side of the vein ; be-
 “ yond this the granite resumes its usual ap-
 “ pearance. Sometimes a shistose rock is chan-
 “ ged into a soft clay, which forms a border
 “ some inches thick along the sides of the
 “ vein *.”

It is obvious, that these are changes precisely
 opposite to what would have been produced by
 heat, while they are such as might have origi-
 nated from the percolation of a fluid, and are
 those, indeed, which are produced by exposure
 to moisture. If we should, therefore, refer the
 change produced by veins of whin on the strata
 of sandstone to a different cause, we should at
 once lose all unity of theory, and relinquish a
 principle capable of affording a satisfactory ex-
 planation. The appearance in sandstone is one
 which may justly be ascribed to the same cause
 that has produced the change in the granite or

* Journal des Mines, No. xviii. 85.

gneifs ; and its particular nature may be ascribed to the peculiar action of the percolating fluid, and the nature of the sandstone itself. The contrast between the two hypotheses is evident : the Wernerian explains from one principle *all* the alterations in the rocks contiguous to veins ; the Huttonian explains only *one species* of alteration, while there are others not only inexplicable, but inconsistent with the supposition by which that *one* is solved.

In some cases also it appears that the formation and filling of veins have been nearly simultaneous with the consolidation of the strata ; and in such cases, Werner has remarked that the *gangue* participates considerably of the nature of the rock, and the rock in its turn is more impregnated with the matter of the vein. If this has been the case with some of the veins of whin in the strata, the intermixture, and the induration arising from it, might be more complete.

The induration which is observed where beds of whin come in contact with strata of sandstone, may be explained on the same principle ; either from the percolation of the fluid loaded with particles of whin, or from a degree of intermixture from a formation nearly simultaneous. The same explanation may be given of the induration of fragments of sandstone sometimes found

in veins of whin, as these are in a situation in which this percolation must have taken place to the greatest extent.

Another fact may be added, which proves that this induration must in all these cases be ascribed to some such cause: It is, that sandstone is often highly indurated when in contact with substances where the cause assigned by the Huttonian geologist,—the application of a strong heat, could not have acted. Sandstone much indurated, it is observed by Mr. Jameson, is often found covered by clay and wacken*: and it will not be affirmed that clay, by any heat it ever had, could cause the induration of the stratum of sandstone on which it is incumbent, while by the operation pointed out in the preceding explanation, such an effect may have been produced.

Dr. Hutton observed that where veins of whin run into strata of coal, the coal contiguous to it is frequently found changed in its properties. “ It has lost its fusibility, and appears to be reduced nearly to the state of coke or charcoal:” a change which is ascribed to the melted whin having by its heat expelled the bituminous matter of the coal. The fact does not always correspond with this observation, but is sometimes

* Nicholson's Journal, vol. iii.

the reverse. But where it does occur, it may, in common with other alterations of the strata by veins running through them, be ascribed to the operation of the fluid by which the vein was filled. This percolating through the coal might be capable of producing changes in its composition and properties; it might deposit a portion of earthy matter which would render the coal less inflammable: and when we consider that a similar cause can occasion the decomposition of feldspar, mica, and hornblende, there is no improbability in the supposition that it might carry off, or cause the decomposition of the bituminous part of the coal. To either or both causes may be owing the change in the properties of that fossil.

This fact is important in another point of view, as it may probably lead to a demonstration of the falsity of the Huttonian hypothesis. The difference between whin and lava is said to be, that the former has been in fusion under an immense compression, while the latter has been in fusion at the surface; and this difference is stated as the cause of some peculiarities in the one compared with the other. It is given as the cause of the lava being more or less cellular or porous, while the whin is solid, as in the latter the air had been retained, which in the former had escaped; and likewise as the cause of the

whin containing calcareous spar, while the lava does not, the compression present in the fusion of the former having prevented the expulsion of the carbonic acid. It is admitted, therefore, that wherever whin has been in fusion without this compression, it ought to assume the precise characters of lava ; it ought to be more or less porous, and ought to contain no carbonate of lime. Now in the fact above stated, with respect to the coal, it is assumed that compression had not been present, as the coal had allowed its bituminous matter to be expelled. It will not be contended that compression could be removed from the coal, and present on the melted whin, at the distance of a few inches. If therefore any carbonate of lime should be found in the whin in such a situation, either in its composition, or in a separate crystallized state, (and the probability is, that it will be found ;) or even if the whin should not differ somewhat in its appearance from common whin, and approach to lava, we shall obtain unanswerable demonstration of the falsity of some of those suppositions which constitute the Huttonian theory.

Again it is stated, that “ the disturbance of
 “ the strata, wherever veins of whinstone a-
 “ bound, if not a direct proof of the original
 “ fluidity of the whinstone, is a clear indication

" of the violence with which it was introduced
 " into its place *."

But that such appearances are not effects necessarily produced from the invasion of the strata by streams of melted whin, and do therefore not indicate such an invasion, is proved by there being frequently no signs of disturbance whatever accompanying veins either of this rock or of granite. Veins of whin traverse coal without any dislocation of the bed of coal; and veins of granite are often unaccompanied by these marks of violence which might be expected from its irruption into the neighbouring strata. The absence of these in many cases affords an argument at least equivalent in force to their presence in others; and indeed, even where they do occur, it is admitted that they do not prove the original fluidity of these masses.

In the Neptunian system there is no difficulty in explaining all these appearances, and some of them are even stated by Werner as proofs of his theory of veins. According to that theory, the fissures in rocks and strata, which, when subsequently filled, form veins, have in general arisen from an unequal subsidence of the rock in which they are found, one part of it from a difference in its height, in the materials composing it, or in the want of support from contiguous rocks having sunk more than the other. It is

* Illustrations, &c. p. 74.

obvious, therefore, that the rock on the one side, of the fissure produced by such a cause, will be lower than the rock on the other, and hence the shifting of the strata observed by the sides of veins. These sinkings of the strata, producing such fissures, have also taken place at various times, as is proved by several facts in the appearances of veins, particularly by that of one vein crossing another; and from this circumstance the disturbances sometimes present are easily accounted for. If after a vein was formed, a new subsidence had taken place in the rock in which it was situated, it is evident that dislocation, both of the vein itself, and of the contiguous parts, must have taken place to a greater or less extent.

It is to be observed also, as a fact highly favourable to the Neptunian theory of these disturbances, that the fissures where these slips or heavings of the strata take place, are frequently filled with substances, such as soft clay or sand, which cannot be supposed to have been ejected with any projectile force, or to have produced any alteration of the existing arrangement by the violence of their introduction.

It is further stated, that when whin is found interposed in tabular masses between beds of stratified rocks, " it is not uncommon to find " the strata in some places contiguous to the

“whin, elevated, and bent, with their concavity
 “upward, so that they appear clearly to have
 “been acted on by a force that proceeded from
 “below, at the same time they were softened,
 “and rendered in some degree flexible*.” An
 appearance of this kind, however, might equally
 be produced from the subsidence of certain parts
 of the strata while they were soft and yielding,
 as from the elevation of other parts of them;
 and consequently, such an appearance affords no
 certain indication of the nature of the cause that
 has operated on them.

The last argument, from the position of whin,
 is thus stated by Professor Playfair: “If it be true,
 “that the masses of whin thus interposed among
 “the strata, were introduced there after the
 “formation of the latter, we might expect to
 “find, at least in many instances, that the beds
 “on which the whinstone rests, and those by
 “which it is covered, are exactly alike. If
 “these beds were once contiguous, and have
 “been only heaved up, and separated, by the
 “irruption of a fluid mass of subterraneous la-
 “va, their identity should still be recognised.
 “Now, this is precisely what is observed; it is
 “known to hold in a vast number of instances,
 “and is strikingly exemplified in the rock of
 “Salisbury Craig, near Edinburgh.

* Illustrations, &c. p. 75.

" The similarity of the strata that cover the
 " masses of whinstone, to these that serve as the
 " base on which they rest, and again, the dissi-
 " milarity of both to the interposed mass, are
 " facts which I think can hardly receive any
 " explanation on the principles of the Neptu-
 " nian theory. If these rocks, both stratified
 " and unstratified, are to be regarded as pro-
 " ductions of the sea, the circumstances would
 " require to be pointed out which have deter-
 " mined the whinstone, and the beds that are
 " all around it, to be so extremely unlike in
 " their structure, though formed at the same
 " time, and in the immediate vicinity of one
 " another; as also those circumstances on the
 " other hand, which determined the stratified de-
 " posites, above and below the whinstone, to be
 " precisely the same, though the times of their
 " formation must have been very different. The
 " homogeneous substances thus placed at a di-
 " stance, and the heterogeneous brought so
 " closely together, are phenomena equally un-
 " accountable, in a theory that ascribes their
 " origin to the operation of the same element,
 " and that necessarily dates their formation ac-
 " cording to the order in which they lie one
 " above another *."

* Illustrations, &c. p. 75.

The argument is here strongly stated ; but nothing is more easy than to show that it is equally conclusive against the Huttonian theory ; that the difficulty which is urged is a particular example of one more general, which is common to both. In the view which is presented to us, our attention is confined to the alternation of whin with the stratified rocks ; but all that is said applies with the same force to the alternation of the strata with each other. These, according to both theories, are formed in the same manner ; and the general difficulty presents itself, how we shall account for a stratum of a particular kind being formed, for the formation of it being interrupted, while one of a different kind is produced above it ; and for the formation of the first being again resumed, so that it shall be precisely similar to the one beneath, though it is cut off from all communication with it, by the stratum of a different kind which is interposed. This will be rendered evident by an illustration : Among the vertical strata, micaceous schistus and slate frequently alternate with each other ; according to the Huttonian theory, these, previous to their elevation, were in a horizontal position ; the micaceous schistus, suppose, is undermost, and must have therefore been first formed ; over this is the slate ; and again, above this, is the micaceous

shiftus, the same as beneath. In this case, it cannot be supposed that the slate has been introduced in fusion between the two strata of shiftus, for there is frequently an extensive series of alternations, and both of these rocks are considered, even in the Huttonian system, as stratified, and of the same formation. We have here, therefore, the difficulty which Professor Playfair so strongly urges, of accounting for the similarity of strata between which another is interposed, and the dissimilitude of those to this interposed mass. In the strata which remain horizontal, the case is precisely the same. An alternation which not unfrequently occurs, is that of limestone with argillite. The strata of limestone, between which the argillite is interposed, are alike in their properties; yet it will not be supposed that these strata have been divided, by the argillite having been introduced between them in fusion. They must be admitted to be of successive formation; and the argument of the learned Professor may, in this case, be directed with its full force against his own system; for, if they have been formed in the same manner, as is admitted, what cause can have determined the formation and deposition of the argillite, between the formation of the strata of limestone? and how should these latter

the latter has been thrown up in a fused state, and has invaded the others, filling up every cavity. According to this view of the subject, it is evident that the line of distinction between them should always be well marked. A slight alteration might take place in the appearance or hardness of the stratified rock, from the heat of the melted matter brought into contact with it; but there should be no such thing as an imperceptible gradation or transition of the one into the other, while, if both are formed in the same manner as the Neptunian theory supposes, such a gradation is not more than what might be expected occasionally to take place.

This transition, therefore, of the unstratified into the stratified rocks being so unfavourable to the Huttonian hypothesis, is attempted to be denied: " If, in these instances, (says Mr. Playfair) the gradation were insensible, as some have asserted it to be, between the strata and the interposed mass, so that it was impossible to point out the line where the one ended and the other began, whatever difficulties we might perceive in the Neptunian theory, we should find it hard to substitute a better in its room. But the truth seems to be, that, in the cases we are now treating of, no such gradation exists; and that, though where the two kinds of rock come into contact a change is often observed,

“ by the strata having acquired an additional de-
 “ gree of induration, yet the line of separation
 “ is well defined, and can be precisely ascertain-
 “ ed. This is at least certain, that innumerable
 “ specimens, exhibiting such lines of separation,
 “ are to be met with; and wherever care has
 “ been taken to obtain a fresh fracture of the
 “ stone, and to remove the effects of accidental
 “ causes even where the two rocks are most
 “ firmly united, and most closely assimilated, I
 “ am persuaded that no uncertainty has ever re-
 “ mained as to the line of their separation. For
 “ these reasons, it seems probable that the gra-
 “ dual transition of basaltic into the adjoining
 “ strata, is in all cases imaginary, and is, in
 “ truth, a mere illusion, proceeding from hasty
 “ and inaccurate observation *.”

The observations, however, by which the
 transition of the unstratified into the stratified
 rocks are established, rest on authorities too un-
 exceptionable to be overturned by an assertion
 of this kind. They are established on the au-
 thority of Werner, allowed to be the most skil-
 ful mineralogist of his age, and whose observa-
 tions, because they run counter to the views
 of the theorist, are not to be termed hasty and
 inaccurate, or mere illusions; and they are sup-

* Illustrations, &c. p. 44.

ported by the authorities of Saussure, Charpentier, Reufs, and other geologists of the highest character. Professor Playfair indeed, in a great measure, recalls, in a subsequent part of his work, the assertion that has been quoted. "I am disposed," says he, "to make some limitation to what is said in § 72, where I have expressed an absolute incredulity as to such transitions as are here referred to. The great skill and experience of the mineralogist who has described the strata at Scheibenberg, do not allow us to doubt of his exactness, though some of the appearances are such as decomposition and wearing might well enough be allowed be supposed to produce." The manner in which this acknowledgment is expressed sufficiently shows the reluctance with which the fact is conceded. It is indeed one which bears with the utmost force against the Huttonian hypothesis of the unstratified rocks, and which is but feebly obviated by an improbable supposition.

The observations of Werner establishing this transition of basalt into stratified rocks, are of the first importance, and his reasoning from them convincing and just. At the bottom of the hill of Scheibenberg, he observes, "there is first a thick bank of quartz sand; above that, a bed of clay; then a bed of the argillaceous stone

“ named wacken; and upon this lay the basalt.
 “ When I saw the three first beds running almost
 “ horizontally under the basalt, and forming its
 “ base, the sand becoming more fine above;
 “ then argillaceous, and at length changing in-
 “ to real clay; as the clay was converted into
 “ wacken in the inferior part; and finally the
 “ wacken into basalt;—in a word, when I found
 “ a perfect transition of pure sand into argilla-
 “ cious sand, of this into sandy clay, and of the
 “ sandy clay, through many gradations, into fat
 “ clay, wacken, and lastly basalt,—I was irre-
 “ sistibly led to conclude, (as every impartial
 “ judge, struck with the consequences of that
 “ phenomenon, would have been) that the
 “ basalt, wacken, clay, and sand, are of one
 “ and the same formation; they are all the ef-
 “ fect of a precipitation by the humid way, dur-
 “ ing one and the same submersion of this coun-
 “ try; the waters which covered it brought at
 “ first the sand, afterwards deposited the argil,
 “ and changed gradually their precipitation in-
 “ to wacken, and lastly into true basalt.”

After again stating some remarks, to ren-
 der doubtful, if possible, the observations of
 Werner on these transitions, which shew the
 unwillingness with which they are admitted,
 Professor Playfair proposes an hypothesis, to ac-
 count for them on the Huttonian system: “ It

" is certain," says he, " that the basis of whin-
 " stone, or the material out of which it is pre-
 " pared by the action of subterraneous heat, is
 " clay in some state or other, and probably in
 " that of argillaceous shistus. It follows of
 " consequence, that argillaceous shistus may,
 " by heat, be converted into whinstone. When,
 " therefore, melted whinstone has been poured
 " over a rock of such shistus, it may, by its
 " heat, have converted a part of that rock in-
 " to a stone similar to itself; and thus may
 " now seem to be united, by an insensible gra-
 " dation, with the stratum on which it is in-
 " cumbent; and phenomena of this kind may
 " be expected to have had really happened,
 " though but rarely, as a particular combina-
 " tion of circumstances, seems necessary to pro-
 " duce them*."

The conclusions here follow each other with
 great rapidity, as if they were obvious and un-
 deniable; yet the whole is a series of hypotheses
 brought forward to reconcile, if possible, the
 fact of the gradation of these rocks with the
 Huttonian system, but unsupported by any
 proofs, and even improbable in themselves. It
 is not proved, or rendered probable, that whin
 is formed by fire from argillaceous shistus. On

* Illustrations, p. 285.

the contrary, the composition of basalt is peculiar, as according to the analysis of it by Dr. Kennedy, it contains a quantity of soda, which is comparatively a rare ingredient in the mineral kingdom. Though it were proved, it is not argillaceous shistus, but unconsolidated clay that the basalt could have come into contact with in these strata; and let Professor Playfair endeavour to establish his hypothesis, by converting clay into basalt by the application of heat. Nor, lastly, if the circumstances were of the most favourable kind; if even the stratum with which the basalt was in contact, was actually the material from which basalt could be formed; the heat of the stratum of fused basalt, supposed to be introduced, could not be sufficient to convert a large bed of it into wacken. We know that the heat necessary to fuse basalt is very considerable; we observe, that at the junctions of whin with the strata, there is often scarcely any change, and where any is perceptible, it does not extend above a few inches. What circumstances, therefore, could have been present, which enabled this stream of fused basalt to convert, by its heat, a large bed of clay into wacken, into which it imperceptibly graduated. If basalt, in a state of fusion, is capable, by its heat, of converting clay into a substance analogous to itself, it is obvious that the strata

which are in contact with it, should always be materially changed; and, above all, if this hypothesis be true, no such arrangement could possibly exist, as a bed of unaltered clay or shistus beneath, and in contact with a stratum of basalt. Yet such connections of these strata are not unfrequent. Bergman, for instance, states an example of basalt incumbent on thin beds of clay or bituminous shistus; and Kirwan, and other mineralogists, mention instances of trap alternating with argilite. The Huttonian, therefore, is reduced to the dilemma of failing to account, either for the gradation of clay into basalt, or the contact of this rock with unaltered clay. Lastly, the general gradation observed in this mountain is overlooked. It is not confined merely to that of the clay into the wacken, but extends from the sand to the basalt, through the whole series; and it is transgressing obviously the rules of just reasoning, to confine the explanation to one part of it merely, and ascribe this to a local cause, or to refer that part to one cause, and the rest to another.

Neither is this the only example of the gradation of unstratified into stratified rocks. It is also observed occasionally in granite; and is indeed a geological truth, of which there can be no doubt. In the Neptunian system, as both these classes of rocks are supposed to be of simi-

lar formation, the phenomenon is not surprising, but, in the Huttonian hypothesis, it cannot be explained. Their formation is stated to be entirely different. The matter of the unstratified mass is supposed to be thrown up in a fused state among the strata; it is applied to them in this state, and may therefore be united to them, but it is in direct contradiction to such a system, that the one should insensibly graduate into the other. In every hypothesis, a number of the phenomena to which it relates, will be apparently explained; but if its basis is not in truth, there will always be some which cannot be brought under it, but stand in opposition to its assumptions, proving their fallacy. Such a fact, with regard to the present hypothesis, is the gradation of the unstratified into the stratified rocks; and so clear are the indications it affords of the origin of these masses, that it is with justice it has been considered as one of the strongest proofs of the Wernerian system. It gave a deadly blow to the theory of the Vulcanists, and it inflicts one not less fatal to the Plutonic hypothesis.

Another fact scarcely less unfavourable to the Huttonian hypothesis of the formation of these rocks is, that they are not unfrequently, both granite and trap, found stratified. The stratification of granite, both horizontal and verti-

cal, is established by the observations of Saussure, and other mineralogists, and is admitted by Mr. Playfair; yet, as granite, according to the system he defends, consists of matter in perfect fusion, thrown up from the central regions, we do not see how it could have formed any other than an irregular mass, and we might with as little reason expect to find stratification in it as in a bank of lava.

This difficulty is attempted to be removed by the following explanation. “ Rocks, of which
 “ the parts are highly crystallized, are already
 “ admitted as belonging to the strata, and are
 “ exemplified in marble, gneiss, and veined granite. In the two last we have not only stratification, but a schistose, united with a crystallized structure, and the effects of deposition
 “ by water, and of fluidity by fire, are certainly no where more singularly combined. The
 “ stratification of these substances is, therefore,
 “ more extraordinary than even that of the most
 “ highly crystallized granite. Neither the one
 “ nor the other can be explained, but by supposing, that while such a degree of fluidity
 “ was produced by heat, as enabled the body
 “ when it cooled to crystallize, the whole mass
 “ was kept in its place by great pressure, acting on all sides, so that the shape was preserved as originally given to it by the sea*.”

* Illustrations, p. 336.

It is not easy to discover what is here meant by the shape originally given by the sea being preserved. Granite is supposed, in the Huttonian system, to be matter which has been completely fused in the central regions, and erupted ; and if it has undergone these operations of perfect fusion, and of eruption, it cannot surely be imagined that it could have preserved its original stratification. And if it is supposed, that stratified granite may not have been formed in this manner, but that its materials have been fused in the place where they were deposited, the unity of the theory is entirely lost, and two hypotheses respecting the origin of this rock are actually advanced.

It is besides, impossible on any supposition, to believe that matter completely in fusion, let it be subjected to what pressure it may, could preserve its original division into beds. That pressure would not prevent the gravity of the upper parts of the mass from being exerted on the under, and if it was fluid, this pressure must have obliterated every trace of separation. The stratification of granite, therefore, cannot be explained by the hypothesis Professor Playfair has advanced.

The stratification of trap furnishes an objection not less conclusive, and perhaps more striking, as its strata are generally alternated with

others. Of this a striking example has been stated as existing at Habichtswalde, near Cassel :
 " On secondary limestone are found strata of
 " sand, clay, wacken, and basalt, which alter-
 " nate with each other not less than three times,
 " and always in the same order. Over the third
 " stratum of basalt is found a thick bed of coal,
 " which is covered by a quartzose sandstone,
 " containing the remains of plants, and petrified
 " wood. Lastly, on this sandstone, are found,
 " in the same order as before, strata of clay,
 " wacken, and basalt; in the sand are found
 " marine shells; the strata of basalt have a bas-
 " altic *tufa* intermixed with them, containing
 " fragments of basalt, olivin, and vegetable re-
 " mains *." Reuss and Dolomieu have also ob-
 served strata of basalt, alternating with strata of
 limestone.

The argument which this disposition of trap furnishes against the Huttonian theory, of its formation, is very evident. It is supposed to have been thrown up among the strata, and to have filled any cavities or spaces between them. In this manner, it might be conceived, that a rock, or mountain of trap, might be formed. But, is it possible that it could have formed strata alternating with others? What are the suppositions necessary to account for such an ar-

* Traité de Minéralogie par Brochant, tom. II. p. 609.

rangement as that stated above?—that a stratum of limestone should have existed, then a bed of clay, another of sand, and over this an empty space, corresponding in size and direction to the strata beneath; that this arrangement should be repeated three times; and that these empty spaces should have afterwards been filled by fused basalt and wacken injected into them. It may at once be affirmed, that such suppositions are inadmissible; and that were they made, their extravagance would afford a sufficient refutation of the system that required them. If it should be said, that, in such cases where trap is stratified, it is formed in the same manner as the other strata, by its materials having been deposited, and softened or fused; this is at least relinquishing the simplicity of the theory, and proposing two modes of formation of this rock. And even this sacrifice would prove inadequate, for the above arrangement in which *sand* and *clay* alternate with the others, completely excludes the operation of fire, and proves that the whole must have been formed by the agency of water.

Lastly, the remains and impressions of organic substances found in trap demonstrate its aqueous origin. In this country, in which this rock is abundant, such appearances are undoubtedly rare, and indeed have not been observed. But in other countries, they appear to be far from

being uncommon. Werner found in a vein of wacken, at a depth of not less than 150 fathoms, trees, with the branches, and even leaves, petrified; and it is stated, on the same authority, that different rocks of the trap kind, contain marine shells, and even bones of quadrupeds. Nothing can be more obvious, than that such substances could never have been contained in a stream of melted matter thrown from the central regions: or if they had even fallen into it after its irruption, and while still fluid, as it is supposed may have been the case, they must have been destroyed by the fused matter; and indeed the circumstances, in some of these cases, are irreconcilable with such a supposition.

From this review of the Huttonian theory of the unstratified rocks, it must be evident, that it is attempted to be supported by appearances which admit of an equal, and in some cases a more satisfactory explanation, from the opposite opinion. The occasional stratification of these rocks, their gradations into those which are stratified, and their alternations with other strata, are inconsistent with that opinion, and indubitably prove their aqueous origin.

Under this class of arguments, may be considered those drawn from the appearances of veins, the substances filling their cavities falling

properly under the description of unstratified minerals.

Veins are accurately defined by Professor Playfair, "separations in the continuity of a rock, of a determinate width, but extending indefinitely in length and depth, and filled with mineral substances different from the rock itself*." It is admitted by mineralogists, that these veins are subsequent in formation to the rocks or strata in which they are found; and the crystallized state of the substances with which they are commonly filled demonstrates that these had been introduced in a fluid form.

The Huttonian theory of veins is, that they have been formed by injection, that the matter filling them has been thrown up in a state of fusion, and that this fused matter entering the rents and cavities of the strata, has consolidated and produced mineral veins.

According to the Wernerian theory, the cavities of veins have been originally fissures in the strata or rocks, produced while these were yet soft and covered by the waters, by various causes, principally by the unequal sinkings of these masses at their consolidation, occasioned by the various densities of the substances com-

* Illustrations, p. 57.

posing them, and their unequal elevations, and by the diminution of the waters by which the mechanical support afforded to the mountain at its sides was withdrawn. The appearances of veins are precisely similar to what might be expected to be the appearances of fissures produced from such causes, as, in their taking a direction approaching more to the vertical than horizontal, in their proceeding without much inflection, in the diameter of them diminishing after they have proceeded a certain extent, and in their at length disappearing entirely, by the approach of their sides, while they generally continue open to the surface, though of a diminished size.

Into the fissures arising from these causes, the fluid still holding much matter dissolved, and covering the surface, would of course find access; and the solution in these cavities being at rest, the crystallizations from it of the dissolved matter, would take place with more regularity, and they would be more completely separated from each other, than in the formation of the strata. Many of those veins have also been filled by successive crystallizations, and hence a greater variety of substances have been introduced into them.

The formation of these veins is of very different dates, the fissures from which they arise having happened at different times. This is evident, from the simple fact, that one vein of-

ten crosses another without interruption or alteration of the matter it contains. A new fissure has taken place traversing the existing vein, and has been afterwards filled, forming a new vein, and this, it has been proved by repeated crossings, has taken place a number of times successively. Such repeated fissures often derange the direction of the vein, and its relative situation with respect to the sides of the rock in which it exists.

Several of the arguments in support of the Huttonian theory of mineral veins are drawn from the nature and properties of the substances they contain, such as their insolubility in any one menstruum, and the mutual impressions in their crystallization. These are afterwards to be noticed; at present we are to consider those deduced from the structure and positions of the veins.

That the matter filling the veins has not been introduced in a state of solution, is evident, it is alleged, from their being no trace of that solvent in the vein, and from the vein itself being completely filled up.

But, if the entrance into the vein has been open, as is maintained, these appearances are not different from what might be expected. Consolidation, or crystallization, it has already been shown, may take place from solution, without

any sensible portion of the solvent being retained; and the matter, consolidating in an open cavity, would exclude the fluid separated, while if a fresh quantity of the solution had access, the cavity might be completely filled up. Or, if the fluid was left, it would penetrate the surrounding matter by infiltration, and leave the vein only partially filled.

It is said, "if the veins were filled by deposition from above, we ought to discover in them such horizontal stratification as is the effect of deposition from water." The usual structure of a mineral vein is that of incrustations, or parallel coats, on its sides. One substance, calcareous spar, for example, or quartz, adheres immediately to the sides of the rock, and next to this a mass of any metallic ore, and these may be variously intermixed with each other, and even with other substances. But, if the Neptunian theory were just, it is affirmed, that the materials should be disposed in horizontal layers across the vein, instead of being parallel to its sides. "On no supposition," it is said, "can these incrustations be received as a proof of aqueous deposition: It may, indeed, be certainly inferred from them, that the matter which they consist of, was fluid at the time of their formation; but the absence of all appearance of horizontal disposition in any part

“ of the vein, amounts nearly to a demonstra-
 “ tion, that this fluidity did not proceed from
 “ solution in a menstruum.” And again, “ if,
 “ as the Neptunists maintain, the materials in
 “ the veins were deposited by water in the most
 “ perfect tranquillity, it is wonderful that we do
 “ not find those materials disposed in horizon-
 “ tal layers across the vein, instead of being pa-
 “ rallel to its sides ; and it seems very unac-
 “ countable, that the common strata, deposited
 “ as we are told while the water was in a state
 “ of great agitation, have so rigorously obeyed
 “ the laws of hydrostatics, and acquired a paral-
 “ lelism in the planes of their stratification, which
 “ approaches so often to geometrical precision ;
 “ while the materials of the veins, in circumstan-
 “ ces so much more favourable for doing the
 “ same, have done nearly the reverse, and taken
 “ a position often at right angles to that which
 “ hydrostatical principles require. This is a pa-
 “ radox, which the Neptunian system has creat-
 “ ed, and which therefore it is not very like-
 “ ly to resolve*.”

This objection it is very easy to remove ; and
 it is somewhat singular that Professor Playfair
 should have stated it in such strong terms, and
 as even amounting nearly to a demonstration
 against the Neptunian theory, when the answer

* Illustrations, &c. p. 251, 253.

is so obvious and satisfactory. The matter filling veins is perhaps always crystallized, or has the crystalline structure, so as to shew that it has been by this species of consolidation that it was formed. Now crystallization is always promoted by a *nucleus* or support, and in any cavity, invariably takes place from the bottom and sides. In the case of a vein therefore filled with a solution of different kinds of matter, the substance most disposed to crystallize would first form an incrustation on the sides and bottom, and afterwards, those which had less disposition under the circumstances of the case to crystallize. According to these circumstances too, the substances present might be variously mixed. Werner states this very fact of the incrustation of the materials of veins on their sides as a proof of his theory, and particularly, that the incrustations on one side are always alike and disposed in the same order, as on the other side of the vein. It is remarked also, as highly favourable to the same theory, that the different coatings are of a greater thickness at a depth in the vein, than they are nearer to the surface.

If, on the other hand, the vein had been filled with different kinds of matter in fusion by injection, it is evident, that these ought immediately to begin to arrange themselves in the order of their specific gravities; the consolidation

of them from cooling could not commence immediately ; and therefore, while still fluid, the heavier substance, the metal, for example, should fall towards the bottom, and the much lighter matrix rise to the top, a disposition which is never observed : And, when they began to concrete, it is equally obvious that the one least fusible should first become solid, while the more fusible remaining fluid, would be forced aside and collected apart ; and thus there could be none of that intermixture, and those mutual impressions, which are generally observed in the materials of a vein ; nor could the cooling of that part of the matter which was next to the sides of the vein, (a cause stated by Mr Playfair), have much effect in preventing this, or in disturbing the regularity of consolidation. This hypothesis, therefore, does not account for the appearances which veins actually exhibit.

It may also be observed, as an important fact in geology, that veins have occurred in which the depositions are horizontal. De Luc gives an example of this kind. The fact is utterly inconsistent with the Huttonian theory ; while it proves clearly that the vein has been filled by deposition from above, the deposit being probably more mechanical than chemical, and therefore taking this form.

It is stated as an argument in favour of the

Huttonian account of the formation of veins, that they "contain abundant marks of the "most violent and repeated disturbance," shifting of the strata, and shifting or heaving of the vein itself.

These appearances, however, are likewise very easily explained by the Wernerian theory: the explanation has indeed been already stated, in showing the fallacy of the argument for the igneous origin of whin, from the derangements in the strata which accompany its veins. All veins arise from fissures in the strata or rocks in which they are situated; these fissures have arisen from unequal sinkings of these strata, soon after their formation, and have taken place successively. It is obvious, therefore, that they must often be accompanied with those very marks of disturbance enumerated in the objection, and of course, that these do not prove veins to have been formed by an irruption of fluid matter from beneath.

Werner has stated the principal varieties of dislocation which attend veins, has shown that they admit of the most satisfactory explanations from his theory, and that the conclusions suggested by the theory, lead to important practical applications in the art of mining. An order is observed even in these derangements, which may be connected by a principle; while,

were the Huttonian hypothesis true, there should be nothing but ruin and disorder.

“ The fact of pieces of rock being found insulated in veins, is certainly favourable,” it is said, “ to the notion of an injected and ponderous fluid having originally sustained them.” Admitting that such pieces rest in no part on the sides of the vein, the phenomenon is very easily explained, from the fact, proved by various appearances, and admitted in the Huttonian theory, that veins have frequently been filled, not entirely at once, but successively ; the substances next to the sides have been first deposited, and those in the middle of the vein have often been of more recent formation. It is obvious, that if, after a partial incrustation of the vein, by which its diameter would be diminished, fragments of rock were brought, by the circulation of the surrounding fluid, or detached by some violent sinking, or subversion of the rock itself, and introduced into the cavity, they might be sustained by the sides, and the vein being afterwards filled up by new depositions, the appearance of an insulated fragment would be produced. It is also possible, that after a vein has been filled, and completely consolidated, a new fissure might take place in it, preserving the same direction ; and pieces of rock falling into it, from either of the above

causes, might be cemented by new matter deposited from water; and in this manner Werner has explained the origin of cemented fragments of this kind which occur in some veins.

A fact in the structure of veins incompatible with the Huttonian hypothesis, and proving the Wernerian theory, of their having been filled from above, is that of petrifications, marine shells, and even vegetable substances, being frequently found in the substances filling the vein, a fact established on the authority of Werner, and others, and even allowed by Dr. Hutton*, nor can there be imagined any more decisive. As the matter with which veins are filled is supposed, by the Huttonian, to be thrown up in perfect fusion, it cannot be conceived that such substances should be brought up from the central parts of the globe in this stream of melted matter, or that if they had fallen into it, they should have suffered no change; while the explanation of their origin is obvious and natural, when we suppose the matter in which they are inclosed to have been deposited from the sea.

An argument of a similar kind may be derived from the fact, that veins are sometimes filled with substances which obviously could never have been in fusion. Thus Werner men-

* Theory of the Earth, vol. i. p. 396.

tions a vein at Riegelsdorf, in Hesse, the materials of which are nothing but sand and rounded stones. And Schreiber cites a vein in a mountain near Allemont, filled with an argillaceous earth and rounded fragments of gneiss, and which intercepts the metallic veins *. Nothing can more certainly prove, that veins are filled from above, and not formed by irruptions from beneath.

The alteration and decomposition of the rock at the sides of the vein, have been already noticed, as being explained more satisfactorily by the Neptunian than the Huttonian theory. The decomposition of granite, hornblende, and gneiss, into a clay or soft earth, for several inches by the side of the vein, cannot be explained, but from the percolation and chemical action of a fluid which has filled the vein.

In the connection of the contents of veins with certain strata, an order is observed, inexplicable in the Huttonian theory, but satisfactorily explained in the Neptunian. This has been traced by Werner. Tin is never found but in primary strata, principally in granite. Molybdena and Tungsten are found in the same situations, and of course have been formed at the same period. Uranium and Bismuth, though

* Journal des Mines, No. xviii. p. 71.

perhaps of a formation rather less ancient, appear never to be found in stratified mountains. Gold and Silver are sometimes found in the latter, though rarely. Mercury, the grey ore of Antimony, and Manganese, are discovered both in primitive and secondary mountains. Copper, Lead, Zinc, and especially Iron, belong to all the ages of the world. Cobalt and Nickel, are generally of recent formation. There is the same difference to be observed in the substances which accompany the metals. Felspar, horn, the topaz, and the beryl, are considered by Werner as the most ancient. Quartz belongs to all periods. Among the calcareous substances, the most ancient are fluor spar, and apatite. Trap is of much more modern formation, and gypsum one of the most recent *.

Now it must be apparent, that the Huttonian hypothesis of the origin of veins can furnish no principle by which any order of this kind can be explained. These materials are supposed to be thrown from the central regions, in which they had existed in a state of fusion, and their irruption must have been merely accidental. No cause can possibly be imagined why certain metals should have been thrown up only in those rents which were situated in the primary rocks; others

* Journal des Mines, No. xviii. p. 97.

in the secondary, and a third class in strata of both kinds : but in the Neptunian system a principle can be discovered by which this may have been regulated. The deposites from the chaotic fluid, it is proved, were successive, and it must, of course, have happened that some kinds of matter would be formed and deposited at one period, others at another, from the play of affinities exerted, and the force with which they were held in solution. In other words, the same cause which determined granite to be first formed, may have determined the formation or deposition of tin, molybdena, and tungsten, at the same time ; and this principle may be extended to the production of all the others. It is a merit, in a theory, of no trivial importance, that it should thus be able to connect, by one principle, facts of the first importance, but apparently so difficult to be explained.

Lastly, there are veins to which the Huttonian hypothesis cannot possibly apply—those which are included in rocks, and shut in on all sides. These cannot be supposed to have been filled by injection, as the termination of them in the rock is obvious. Professor Playfair is obliged to relinquish the general theory, and to suppose that when “ these veins are found in stratified rocks, such as “ have not themselves been melted, we must conceive them to be composed of materials more

" veins, yet the time was when these materials
 " were all mingled together in the chaotic mass,
 " and constituted one vast fluid, encompassing
 " the earth; from which fluid it was that the
 " minerals were precipitated, and deposited in
 " the clefts and fissures of the strata *."

After this declamation, it is unfortunate that the Huttonian should be compelled to admit that there are examples of veins filled with metallic matter, cut off from that source from which only, according to Dr Hutton's explanation, they could have been filled, and of which no probable explanation can be given but that by the Neptunian theory. It is even probable that all veins are of this insulated kind, their limits not being always discovered, from their not being explored to sufficient depth; for it is, *prima facie*, an extreme improbability that rents should pass through innumerable strata even to the central parts of the globe, and that these should be filled by injection to the very surface, and through crevices often only a few lines in diameter for a considerable length.

An appearance not uncommon in veins is that of their becoming narrower as they descend, often lessening to $\frac{1}{4}$ of an inch or less in thickness, and either remaining so or again becoming wider,

* Illustrations, &c. p. 248.

while above, they are perhaps some feet in width. It is obviously impossible that the matter filling this upper part should have been thrown contrary to its own gravity, by injection, through such a passage, from the central parts of the globe.

PART IV.

Of the Support which the Huttonian and Neptunian Theories derive from the Appearances and Properties of Individual Fossils.

THIS part of the investigation of these theories, from induction, is that perhaps from which we may derive the most conclusive evidence. The positions and connections of the strata, can, in many cases, be only imperfectly observed, and they are also often such as might have arisen from various causes. But it is reasonable to presume, that in individual minerals properties will be found which shall afford undoubted proof whether they have been formed by water or by fire.

In considering the examples of this class, brought forward in proof of the Huttonian theory, I shall take them in the order in which they are stated by Professor Playfair. The first are those belonging to the SILICEOUS GENUS.

“ Fossil-wood, penetrated by siliceous matter,

“ is a substance well known to mineralogists;
 “ it is found in great abundance in various si-
 “ tuations, and frequently in the heart of great
 “ bodies of rock. On examination, the filice-
 “ ous matter is often observed to have pene-
 “ trated the wood very unequally, so that the
 “ vegetable structure remains in some places
 “ entire; and in other places is lost in a homo-
 “ geneous mass of agate or jasper. Where
 “ this happens, it may be remarked, that the
 “ line which separates these two parts is quite
 “ sharp and distinct, altogether different from
 “ what must have taken place, had the flinty
 “ matter been introduced into the body of the
 “ wood, by any fluid in which it was dissolved,
 “ as it would then have pervaded the whole,
 “ if not uniformly, yet with a regular gradation.
 “ In those specimens of fossil-wood that are
 “ partly penetrated by agate, and partly not
 “ penetrated at all, the same sharpness of ter-
 “ mination may be remarked, and is an ap-
 “ pearance highly characteristic of the fluidity
 “ produced by fusion*.”

From the appearance of fossil filiceous wood,
 the unbiaffed observer would be much more
 ready to infer, that it had been petrified by the
 operation of water, since it is scarcely possible
 to believe that it could be subjected to the ac-
 tion of fire, and at the same time have preserv-

* Illustrations, &c. p. 25.

and, instead of affording any support to his opinion, it is an additional difficulty, which he is unable to remove. The appearance seems to be owing to the process in the humid way going on very slowly, probably from the density of the wood, and to its having been stopt in those specimens before it was complete. It is conceivable that the external part of the wood may have been so completely impregnated with the siliceous matter as to prevent the infiltration of the water to the internal parts, and of course the process of petrification would cease, and this, if the wood has been very dense, it is possible may have proceeded only a short way, and have been abruptly terminated.

There is another fact, with respect to siliceous wood, which gives indication of its watery origin, while it is inexplicable on the Huttonian hypothesis; it is, that of shells often adhering to, and even indented in the wood, deprived of their calcareous matter, and thus forming siliceous petrifications. The presence of these proves that the wood had been immersed in water; and the entire conversion of their substance into siliceous matter cannot be accounted for on the supposition of melted filix being applied to them; for by what power was this filix to expel the calcareous carbonate, of which they principally consist. On the opposite opinion

this admits of explanation, the carbonate of lime, being more soluble in water than the siliceous earth, would be gradually carried off, and the latter might be deposited in its place.

In favour of the petrification of siliceous wood being effected by the medium of water, we have the analogous case of the petrification of wood in this way by calcareous matter. There are many springs, in which a portion of carbonate of lime is held in solution, which have the power of petrifying any vegetable substance thrown into them; and many examples of petrified mosses and other matters are to be found, which no one can suppose to be performed by fire.—Here the ultimate effect is the same. The only difference is, that in the one case the petrification is siliceous, in the other calcareous. Does not the presumption follow, that the process by which it has been effected is similar; that in the one, calcareous matter dissolved in water; in the other, siliceous matter in solution has been applied to the petrified substance?

Lastly, we have demonstration that siliceous petrified wood is formed in the humid way. Mr. Kirwan relates a decisive proof of this kind: one of the timbers supporting Trajan's bridge over the Danube being taken up, and examined, was found to have been converted into agate to the

depth of half an inch, while the inner parts were more slightly petrified *.

With such weight of evidence, the conclusion cannot be resisted, that siliceous wood has been petrified by the medium of water ; and this first proof of the Huttonian theory, from the properties of minerals, serves to establish the opposite system.

This argument with respect to the siliceous petrification of wood, is of more importance than at first view may appear. It is not merely in itself an example which may be brought in support of the one theory or the other, neither is its importance confined to its proving the solubility of siliceous earth in water, and the possibility of the most perfect consolidation being affected by its deposition ; but it establishes a similar formation with regard to other fossils, and furnishes a proof capable of being carried to a considerable extent. It is observed in specimens of wood thus changed, that where there are rents or vacuities in the wood, the siliceous matter deposited in these, has always assumed the figure and structure of agate. It has the concentric coats of that fossil, its hardness, frequently its various shades of colour, and in short, all its properties. If, there-

fore, the petrified wood is proved to be formed in the humid way, it follows, that agates may be formed in the same mode; and this fact again may be pushed still farther, for agates are almost always found inclosed in other rocks, as, for example, in trap, and inclosed in such a manner as to render it undoubted that the rock and the inclosed agate must have had the same origin. So far, therefore, may the application of this argument be carried, from the intimate connection of these fossils. Dr. Hutton might perceive this connection, and the obligation it laid him under of ascribing the formation of filiceous petrified wood to fusion, since, if he admitted it to have been formed in the humid way, he must have been forced to admit, that agates and the rocks in which they are inclosed, might have had the same origin. It was, perhaps, a proof of polemical skill to assume that as an argument which he might have otherwise been obliged to obviate as an objection; and this might lead him to maintain, that filiceous petrified wood was formed from the introduction of fused filiceous earth, though its appearance is, *prima facie*, inconsistent with that opinion, and though it is fully refuted by facts.

“ The round nodules of flint that are found in chalk, quite insulated and separate from one another, afford,” it is said, “ an argument

“ of the same kind ; since the flinty matter, if
 “ it had been carried into the chalk by any sol-
 “ vent, must have been deposited with a certain
 “ degree of uniformity, and would not now ap-
 “ pear collected into separate masses, without
 “ any trace of its existence in the intermediate
 “ parts. On the other hand, if we conceive
 “ the melted flint to have been forcibly injected
 “ among the chalk, and to have penetrated it,
 “ somewhat as mercury may, by pressure, be
 “ made to penetrate through the pores of
 “ wood, it might, on cooling, exhibit the same
 “ appearances that the chalk-beds of England
 “ do actually present us with *.”

This theory of the formation of flint is nearly inconceivable, and is inconsistent with the appearances of that fossil. The kind of injection, by which it is supposed to be introduced into the chalk, is altogether mysterious ; for we perceive not, how, without any sensible openings, the flint is to be conveyed into the chalk, or if it were forced in by some peculiar exertion of pressure, how the particles are to be collected, so as to form nodules of considerable size. The arrangement of these nodules is also incompatible with any notion of this kind. They are not irregularly interspersed in the

* Illustrations, &c. p. 25.

chalk, as they necessarily must have been, if introduced by any species of injection, but are found arranged with the greatest uniformity, in horizontal beds or layers, which are equidistant from each other.

That the formation of these nodules is owing to the agency of water, is proved by the impressions of shells on their internal surface, or even entire shells inhering in the substance of the flint, completely petrified, and having their calcareous matter entirely abstracted. It is obvious, that this petrification and expulsion of the calcareous matter, while the structure and divisions of the shell often remain unaltered, could never be produced by injection of fused siliceous matter, while they may, in common with other petrifications, be easily explained from the agency of water. There is a particular specimen of this kind, one of the most common, which, perhaps, places this in the clearest light,—that of the shell of the *Echinus*, which is frequently found filled with flint, while its texture remains perfectly unaltered; and its calcareous matter is, at the same time, so completely removed, that it does not effervesce on the application of an acid. This shell is, in its natural state, so extremely tender, that the least pressure crushes it to pieces; and it has only a small aperture leading to its cavity. Can it be supposed, that it

“ of the same kind ; since the by any species
 “ it had been carried into the the circumstances
 “ vent, must have been der for such an effect
 “ degree of uniformity, & all should have been
 “ pear collected into that its aperture should
 “ any trace of its e of the stream of injected
 “ parts. On the should have such firmness, as
 “ the melted fil-eration from being completely
 “ among the dense fluid matter ; and were
 “ somewhat in apposition of the existence of these
 “ made admitted, the expulsion of the
 “ woody matter of the shell would remain in-
 “ ar considerable. This, and other petrifications, evi-
 “ shew, that the particles of flint have been
 brought together slowly, and without violence,
 by the agency of water. Mr. Kirwan attri-
 butes their consolidation, to the infiltration of
 that fluid through the strata of chalk * ; and
 their formation into nodules, may be ascribed,
 according to the Wernerian hypothesis, to the
 particles being collected in cavities formed in
 the chalk by the extrication of air.

“ The siliceous pudding-stone,” it is said, “ is
 “ an instance closely connected with the two
 “ last ; in it we find both the pebbles, and the
 “ cement which unites them, consisting of flint
 “ equally hard and consolidated ; and this cir-

* Geological Essays, p. 237.

for which it is impossible to account by the infiltration, or the insinuation of an aqueous solution, is perfectly consistent with the supposition that a stream of melted flint was forcibly injected among a mass of pebbles *."

It seems nothing improbable in the supposition that the pebbles in this fossil have been agglutinated and consolidated by the deposition of the cement from aqueous solution or suspension. Suppose these pebbles to have been placed in a situation similar to that of petrified filiceous wood; its pores have been completely filled with filiceous matter, of the nature of agate; and, in like manner, the interstices between these loose pebbles may have been filled with this matter, and thus a consolidated heterogeneous mass might be formed.

Agates belong to the filiceous genus, and are considered by Dr. Hutton as affording, from their structure, an argument in favour of his theory. They are masses, the internal parts of which have a certain arrangement, which, it is alleged, could not be produced by the infiltration of any fluid, but which must have arisen from the circumstances under which the agate was formed.

* Illustrations, &c. p. 26.

That the fluidity from which agates have been consolidated, has been that of fusion, it is said, is evident, because “ the formation of the “ concentric coats, of which the agate is usually composed, has evidently proceeded from “ the circumference toward the centre, the exterior coats always impressing the interior, “ but never the reverse. The same thing also “ follows from this other fact, that when there “ is any vacuity within the agate, it is usually “ at the centre, and there too are found the regular crystals, when any such have been formed. It therefore appears certain, that the progress of consolidation has been from the circumference inwards, and that the outward coats of the agate were the first to acquire solidity and hardness.

“ Now, it must be considered that these “ coats are highly consolidated; that they are “ of very pure siliceous matter, and are utterly “ impervious to every substance which we know “ of, except light and heat. It is plain, therefore, that whatever at any time, during the “ progress of consolidation, was contained within the coats already formed, must have remained there as long as the agate was entire, “ without the least possibility of escape. But “ nothing is found within the coats of the agate save its own substance; therefore no ex-

“traneous substance, that is to say no solvent,
 “was ever included within them. The fluidity
 “of the agate was therefore simple, and unaf-
 “fected by any menstruum.

“In this argument, nothing appears to me
 “wanting, that is necessary to the perfection of
 “a physical, I had almost said of a mathemati-
 “cal, demonstration. It seems, indeed, to be
 “impossible that the igneous origin of fossils
 “could be recorded in plainer language, than
 “by the phenomenon which has just been de-
 “scribed *.”

Notwithstanding the strong language in which these assertions are expressed, it may be demonstrated, that agates could not have been formed by fusion, but must have originated from the operation of water. Their structure is so peculiar that there may be some difficulty in pointing out clearly the precise mode of their formation, but the following explanation by Werner, and which has also been suggested by Dolomieu and Kirwan, will be found to accord better with the appearances of the fossils of this family, than the Huttonian hypothesis.

During the consolidation of the strata, it is supposed that extrications of air must have taken

* Illustrations, &c. p. 78, 79.

are of precisely the same degree of fusibility. Now there are not, perhaps, two substances in nature with respect to which this is the case. At least, if it happened with any two, it might be considered, both as a singular coincidence, and still more as the result of a very extraordinary combination of circumstances, that of all bodies the two which thus happened to agree in fusibility should have been brought together in fusion. But it may safely be affirmed, that not another example of it would be met with, far less that it should take place with respect to many substances; and if it should actually be found in nature that there were a variety of groupes or aggregates of fossils which mutually impressed each other, this, instead of being regarded as a proof that these substances had consolidated from simple fusion, would furnish the clearest demonstration that the fluidity from which they had become solid must have been of a different kind.

It may be said, perhaps, that it is not clear how from solution two different substances should consolidate or crystallize at the same time. Let it be granted, that it is not obvious how this should happen; yet still we have no demonstration, as in the former case, that this could not be the case; and therefore, as it is the only supposition left to us, it ought to be admitted. But

we need not rest satisfied with this argument. It is possible to conceive how different substances in solution in one fluid may be brought either to crystallize or consolidate together, or to give the appearance of simultaneous consolidation. Let it be imagined, that by some alteration of circumstances in the solution the crystallization of one of the substances was occasioned : it is conceivable that the separation of this substance might so alter the existing attractions, by which one or other of the remaining substances in the solution were held dissolved, that it also might instantly begin to separate, and thus its consolidation would be simultaneous, or very nearly so, with that of the other, and they might impress each other : Or, as crystallization is promoted by a *nucleus*, and still more by the presence of a body already crystallized, it is conceivable, that if in a saturated solution of different substances, one of them should by an alteration of circumstances, be brought to crystallize, the crystallization of the others might instantly commence, as we see take place when we drop a solid into a saturated solution of a salt ; and thus the most intimate admixture and mutual penetration of crystals might take place. Nay, it is no improbable supposition, that by the sudden admission of air to such a saturated fluid, or even by the sudden evolution of any

gas from it, the instantaneous crystallization of more than one of the substances dissolved in it might be produced.

It is thus evident, that various suppositions sufficiently probable can be made, by which the simultaneous consolidation of different substances from a state of solution may be accounted for. It cannot, therefore, be said, that this is a difficulty which cannot be explained, and which presses against the theory. On this point the Neptunian has every advantage over the Huttonian hypothesis. It can be demonstrated, that according to the principles of the latter no such thing as simultaneous consolidation could take place. No demonstration of this kind can be brought against the other, which at once, therefore, gives it the superiority. But it can claim a still higher advantage, since it is even easy to shew, in perfect conformity with its principles, how this singular operation of simultaneous consolidation might take place. Every instance of it, therefore, such as that of chalcidony and calcareous spar in agates, and others to be afterwards noticed, are so many proofs of the formation of fossils by solution.

“ The common grit, or sandstone, though it
 “ certainly gives no indication of having possessed fluidity, is strongly expressive of the effects of heat. It is so, especially in those in-

“stances where the particles of quartz sand, of which it is composed, are firmly and closely united, without the help of any cementing substance whatsoever*.” The Neptunian would say, that this is mere assertion and no argument whatever, as the consolidation of this stone, the density or compactness of which is not great, might be produced by the mutual attraction exerted between its finer particles; or, as all sandstone contains a portion of argil or lime, these might also serve, in part at least, as the connecting medium.

If we should, *a priori*, expect, from an examination of the strata of the earth, any indication of their origin, or of the nature of the agents to whose operation they had been subjected, it is certain that none less ambiguous could be obtained than that afforded by the animal and vegetable impressions and remains which they contain. These at once afford the double proof, that they have been deposited from water, and that they have not been subjected to the action of fire.

This proof we shall find in all the strata, more sparingly perhaps in those of the Siliceous Genus than in the others, but still in those so abundantly as to furnish the most conclusive

* Illustrations, &c. p. 259.

the common cement. Nay, the justness of the assertion in the objection may be denied ; for it is possible, that by the continued application of a solvent to these fragments, they might partly be dissolved, partly softened. They might thus be indented together by pressure, and a cement at the same time formed by which they would be consolidated into one mass.

“ In many other instances it appears certain, that the stones of the calcareous genus have been reduced by heat into a state of fluidity much more perfect. Thus, the saline or finer kinds of marble, and many others that have a structure highly crystallized, must have been softened to a degree little short of fusion, before this crystallization could take place. Even the petrifications which abound so much in limestone tend to establish the same fact ; for they possess a sparry structure, and must have acquired that structure in their transition from a fluid to a solid state *.”

In the first of these instances, there is merely the gratuitous assumption that the crystallization producing the sparry appearances of these marbles, must have been from fluidity induced by fusion, and not from fluidity occasioned by solution. The latter affords an example of a very

* Illustrations, &c. p. 29.

singular mode of reasoning. It is acknowledged, that the petrifications in limestone must have been fused, because they could only acquire their sparry structure in their transition from a *fluid to a solid state*, and of course the figure and the structure of the substances petrified, which are generally shells, must have been lost. But how could this figure be resumed when the fused substance returned to the solid state. If a shell in a mass of limestone had been in fusion, it is obviously impossible that in becoming solid it could again assume its precise arrangement and figure.

The proof from the presence of these petrifications in calcareous strata, in favour of the Neptunian theory, is conclusive. These strata are supposed to be formed in the bed of the sea, and probably chiefly from the decay of marine animals. Their shells consist principally of carbonate of lime; the animal matter which is mixed with this is evolved by putrefaction during their slow consolidation, and its place is supplied by a portion of the carbonate held in solution by the surrounding water: from a similar deposition, mixed with smaller portions of argillaceous earth, oxyd of iron and other substances, their consolidation is completed, and the more slow deposition of pure carbonate of lime, gives rise to the formation of the spar and crystals which these strata always contain.

In the Huttonian theory, the preservation of these animal remains is a problem which is not solved. It is maintained, that consolidation cannot be effected but by fusion, partial or complete; and of course it is maintained, that these strata of limestone and marble have been melted, or at least softened by heat. It remains, then, to be explained, how the shells of the marine animals, which these strata contain, have escaped the action of this heat; how their figure and texture have not been destroyed? Their composition is the same as that of the remaining matter of the limestone or marble; and indeed these strata are supposed, by Dr. Hutton, to originate from the remains of sea animals. If these, therefore, were in a fused or softened state, ought not the included shells to have suffered precisely the same change? The fusion of these strata must, according to the Huttonian, have been nearly complete, for they have the sparry structure, and contain large veins and spots of perfect spar. It is impossible to conceive, how such a degree of fusion should have taken place and these shells not have been at least so far softened, as to have lost somewhat of their figure, and of the sharpness and distinctness of their angles and lines. Nay, the cavity of the shell is often filled with regular crystals of carbonate of lime, which, if formed by heat, must

have concreted from a state of perfect fusion : but this fused matter could not have remained in contact with the shell, without melting it completely or partially ; and, therefore, such appearances, which, so far from being rare, are extremely common, are incompatible with such a supposition.

The conclusion becomes still more evident, when we consider the extreme delicacy often observed in these impressions. The calcareous stone of Monte Bolca is known to every mineralogist, for the large and perfect impressions of different kinds of fish found in it. These are numerous, the entire figure of the fish is impressed upon it, and with such accuracy, that the characters of a number of species have been established. It is absolutely incredible that an impression of this kind could remain on a stone subjected to an intense heat, that the matter of the fish should not have suffered the smallest decomposition, or even mechanical derangement ; and the bare inspection of these specimens will be sufficient to convince the unprejudiced observer, that these strata could not have owed their consolidation to fire : but there is no difficulty in conceiving, that water, by that slow agency above explained, might form such petrifications.

Another fact respecting these fossils, which cannot be explained in the Huttonian hypothesis, has been stated by Mr. Kirwan,—the absence of phosphoric acid in the finer kinds of marble and limestone. The shells of marine animals contain a portion of phosphat of lime in their composition, as has been established by the experiments of Mr. Hatchet; this substance, Mr. Kirwan justly remarks, is indestructible by heat; and, therefore, had these marbles been formed from the remains of marine animals, they ought to contain the full proportion of phosphat of lime, which these contain. The reply which Mr. Playfair gives to this, is extremely obscure. To give the argument force, he observes, it would be necessary to prove, that phosphoric acid exists in these limestones which evidently consist of shells in a mineralized state: if these are found without phosphoric acid, it is evident that the preceeding argument fails entirely. But this proof is not necessary. The marble in which no phosphoric acid has been found, is supposed, by the Huttonian geologist, to be formed from remains of marine animals equally with any other; and the circumstance of other marbles or limestone, containing or not containing phosphat of lime, is of no consequence. If it be found, that other limestones

do not contain phosphat of lime, the argument will indeed become more general; but, however this may be, the objection, from the particular fact with regard to the marbles which have been analysed, remains the same. To the Neptunian theory, the objection does not apply, because the small quantity of phosphat of lime present may be carried off by water.

It is, lastly, to be remarked, with regard to the calcareous fossils, that the evidence for the formation, of at least some of them by water, is so unexceptionable, that it must be admitted even by the Huttonian. Thus, calcareous crystals are often found in situations where they must necessarily have crystallized from a solution of carbonate of lime in water. They are thus met with lining the cavities of shells, such, for example, as the shell of the *Cornua Ammonis*, and many others. These shells themselves consist of carbonate of lime; and of course they could not contain carbonate of lime in perfect fusion, without suffering an alteration. The state of fluidity, therefore, from which these crystals have concreted, has not been that from fusion, but must have been that of solution in water. Another example, not less equivocal, is that of calcareous stalactites. These have not only in general the sparry texture, but are often covered with crystals, and their for-

mation in the humid way is abundantly obvious. The force of this conclusion is evident. It is proved, that calcareous crystals, different in no respect from others found in nature, are, in some cases at least, formed from solution; that they are ever formed by the fusion of carbonate of lime, is a mere hypothesis, supported by no direct evidence;—till such evidence, therefore, be adduced, the Neptunian has a right to regard that which he brings forward as conclusive, and to consider all calcareous crystals as being formed in the same mode; and to this conclusion nothing but a simple possibility can be opposed. Even that conjecture would be, *a priori*, improbable, since it would be singular, if this fossil should be capable of being formed both by fire and water, and should receive from each precisely the same form of crystallization. And, if it be rendered probable, that calcareous crystals are formed by solution, the same mode of formation must be assigned to almost every fossil; for there are very few with which these crystals are not so intimately associated, that the same origin must necessarily be given to them all.

Among the fossils of the ARGILLACEOUS GENUS, the variety of argillaceous iron ore, named the septaria affords an argument on which Dr Hutton laid much stress. This fossil is generally

found in spheroidal nodules, which when broken exhibit a singular structure. The ground or base consists of argillaceous iron ore, but this is divided into a number of septa by veins of calcareous spar, which shoot from the centre of the stone to the circumference, but do not reach it. This proves, it is said, that the calcareous matter has not been introduced into the stone from without by infiltration. "The only other supposition that is left for explaining the singular structure of this fossil is, that the whole mass was originally fluid, and that in cooling the calcareous part separated from the rest, and afterwards crystallized *."

It might be granted, that the structure of this stone proves that it must have been in a fluid state, and that in hardening, the contraction of the base, and the separation of the calcareous matter, had taken place at the same time, without its following as a necessary consequence that this fluidity must have arisen from fusion. It may have been a deposit from water, and the shrinking and separation may have taken place during its consolidation; or it might be supposed, with perhaps equal probability, that the argillaceous iron ore only had been deposited; that in consolidating it had shrunk and split internal-

* Illustrations, &c. p. 26.

ly; and as this is actually a porous stone, if it were afterwards exposed to water having carbonate of lime in solution, the water might enter by infiltration through its substance, and the cavities be filled, as far as they extended, with calcareous matter. That these septaria have had an aqueous and not an igneous origin, is proved by their sometimes containing impressions of organic substances, particularly shells.

In the argillaceous strata, petrifications are extremely frequent, more particularly shells and impressions of vegetables. These are found in clay, argillaceous shistus, argillaceous sandstone, argillaceous ironstone, and various others. The argument with respect to them, is the same as with regard to the other strata in which they are found. The operation of fire ought to have altered or destroyed these remains and impressions, nor is any cause pointed out in the Huttonian theory by which they could be preserved.

This argument is conclusive, whether we consider the deficiency of explanation in the one theory, or the satisfactory solution afforded by the other. Let the example of marine shells preserved in sandstone be taken. If that sandstone has been formed by deposition from water, it might happen that the shells of animals existing in that water, might be involved in the de-

posite ; and, in this case, no change would happen to them, except perhaps their being more or less impregnated with particles of the matter in which they were deposited. This is accordingly the state in which these shells are found, and the actual appearance exactly corresponds with that which the theory would lead us to expect. If, again, the stratum of sandstone were consolidated by heat, it is impossible to conceive how that heat should operate without changing the figure or structure of the contained shells. Nay, in this example, another circumstance is present, which still more forcibly proves, that heat could not thus be applied without producing some change. Argillaceous sandstone consists principally of filiceous and argillaceous earth : now these serve as a flux to calcareous earth, and cause its fusion at a temperature much lower than that requisite to fuse it when pure. Shells of marine animals have this earth for their basis ; and had heat been applied to them, surrounded by argillaceous sandstone, such a combination must have been effected.

Or, let the argument be considered with respect to the vegetable impressions, which are often found on shale or shistus. These are so delicate and perfect, that the genus, and even the species of the vegetable, can often be determined, from the preservation of the most deli-

cate foliage and flower. It may be asked of the Huttonian, if these strata were consolidated by fire, as he supposes, how the vegetable matter could be entirely removed, while the impression should remain perfect and unimpaired? The vegetable matter ought undoubtedly to have suffered decomposition; for, even if the escape of its volatile parts were prevented by the pressure present, still its principles must, under such a heat, have entered into new combinations, by which their structure must have been altered, or, at all events, by the fusion or softness of the stone on which the vegetable matter lay, its figure must have been changed.

Among the BITUMINOUS SUBSTANCES, Coal is brought forward as affording a proof of the igneous origin of fossils. Instead of doing so, it will be found, like the preceding examples, to afford the strongest objections to the Huttonian hypothesis.

From the nature of this substance, and the appearances of its strata, there can be little doubt that it is principally of vegetable origin. The most probable theory of its formation is, that vegetable matter carried to the sea, has, by the direction of currents, been deposited in banks, and that during this submersion it has suffered that slow kind of decomposition by which the greater part of its principles have

been evolved in new combinations, while its carbon, with a portion of hydrogen, have remained ; and this, mixed with more or less earthy matter deposited at the same time from the ocean, has in its soft state been consolidated by the force of aggregation, and has formed coal. The decomposition by which this has been effected, is probably analogous to that which we know animal matter, when immersed in water, suffers. Its hydrogen, azot, and oxygen, enter into various combinations, forming gases which escape ; and its carbon retaining, by a chemical attraction, a portion of hydrogen in combination, remains, forming a species of fat. Carbon is still more abundant in vegetable than it is in animal matter ; and this constitutes the principal difference between them. Vegetable matter, however, is liable to similar decompositions ; and under the circumstances pointed out, it is reasonable to believe that changes of a similar kind, modified as to the result by the difference in the proportions of its principles, will take place : in other words, its residue will be more carbonaceous, but still with a proportion of hydrogen, so as to render it more or less bituminous. We accordingly find, that wood, by immersion in water, becomes first brown, and then black ; and the ligneous fibre, by slow decomposition, is com

pletely converted into a black mould, in which carbon predominates. It is easily conceivable, that this process being carried on under different circumstances, may proceed with various degrees of rapidity, and to a greater or less extent. Hence will originate different varieties of coal, some being much more carbonaceous than others, while their composition is also varied by the different quantities of earth deposited during their formation.

It is not improbable that some species of coal may likewise have a different origin. It may be supposed, and indeed must be, that carbon, in common with other simple substances, existed in the chaotic fluid; and this carbon, combined with a portion of oxygen, may have formed a variety of coal which was precipitated. This probably forms the coal, which is infusible and very little inflammable, the mineral carbon, or anthracite, and which is considered by many mineralogists as very remote from a vegetable origin. From its analysis it does not appear to contain any bitumen, its principles, according to the experiments of Dolomieu and Panzenberg, being pure carbon, or at least oxyd of carbon, silice, argil, and oxyd of iron*: and it is observed by Dolomieu, that this fossil, as well as graphite or plumbago, is found in the primitive mountains,

* Brochant, t. 2, p. 81.

generally in veins. The supposition offered respecting the origin of this variety of coal, is also confirmed by the fact, that carbon is found more or less oxydated in the composition of several primitive fossils and rocks.

Dr. Hutton, however, considers coal as a substance formed by the operation of subterraneous fire; and supposes that there are several appearances connected with it, which prove it to have had such an origin. It is somewhat difficult to give a clear statement of his theory on this subject; but it seems to be comprehended under the following propositions:

First, It is conceived, that an intense heat has been applied to vegetable matter at the bottom of the ocean, and that at the same time no great degree of pressure has been present. By this operation the vegetable matter has been charred, or converted into one species of coal, while the more volatile inflammable matter of the wood has been disengaged. *2dly*, This volatilised matter being of a bituminous nature, is supposed to be diffused in the ocean, and to be “employed in forming other strata, which were then deposited at the bottom of the water.” To this would be added, “all the fuliginous matter that is formed in burning bodies upon the surface of this earth, which is first delivered into the atmosphere, but ultimately must

" be settled at the bottom of the sea *." And, *lastly*, Another supply of bituminous matter is derived from the vegetable substances dissolved or suspended in the water of the rivers, and brought to the ocean. The bituminous matter from these sources is supposed to be precipitated either alone, or with a portion of fine earth, likewise suspended in the water of the sea; and this precipitate, being afterwards consolidated by subterraneous heat, forms strata of pure fossil coal.

It would be an irksome, and a very unnecessary task, to enter on the formal refutation of these various suppositions. A few observations on the most palpable deficiencies of the theory will be sufficient.

It may, in the first place, be observed, that there are no direct arguments adduced to establish the igneous origin of coal. The theory is attempted to be established principally by arguments drawn from its connection with other fossils, which it is to be proved have had an igneous origin. Thus, because " the beds of coal " are disposed in the same manner, and are alternated indiscriminately with those of all the " secondary rocks," it is inferred that they must have been formed by the same operation,

* Theory of the Earth, vol. I. p. 577.

and that this has been fusion by heat, a conclusion founded on nothing peculiar to coal but on the evidence respecting the origin of these secondary rocks. They are also said to be “ traversed like the other strata by veins of all the metals, of spar, of basalt, and of other substances *.” But here likewise the argument is not direct, but depends entirely on its being proved that these veins have been formed by fusion. Lastly, the coal strata are said to “ contain pyrites in great abundance, a substance that is, perhaps, more than any other the decided progeny of fire *.” But this, equally with the preceding arguments, rests on a proof with respect to the origin of a different substance,—a substance which it will immediately be shewn there is every reason to conclude has been formed in the humid way.

From this statement, it is obvious that no direct argument from the properties or appearances of coal, is adduced in proof of its igneous origin.

Not only, however, is it unsupported; several of the suppositions it involves are highly improbable. Thus, it is imagined that a great part of the coal strata are derived from bitumen produced either by the exposure of wood to subterraneous heat, by the burning of vegetable mat-

* Illustrations, &c. p. 33.

ter at the surface, or by the solution or suspension of the oily and inflammable parts of vegetables in the water of rivers ; this bitumen from these sources being supposed to be diffused in the ocean, and precipitated so as to form strata. But how is this matter to be collected in one place, and, if collected, how is its precipitation to be effected. It is lighter than water, especially sea water : it must therefore remain at the surface, and no cause can be assigned for its being carried to the bottom.

The peculiar modifications of pressure supposed requisite for the igneous formation of coal, furnish another argument against the theory. " It must be considered," says Dr. Hutton, " that while immersed in water, and under insupportable compression, the vegetable, oily, and refinous substances, would appear to be unalterable by heat ; and it is only in proportion as certain chemical separations take place that these inflammable bodies are changed in their substance by the application of heat. Now, the most general change of this kind is in consequence of evaporation, or the distillation of their more volatile parts, by which oily substances become bituminous, and bituminous substances become coally *." This, then, is

* Theory of the Earth, vol. i. p. 70.

the supposition which Dr. Hutton chooses to make, and which, indeed, appears to be a necessary one for his theory, that coal is formed by heat applied to vegetable matter under different degrees of pressure, the difference in this respect producing a difference in the result, but that in all cases the pressure must be such as to allow of some separation of volatile principles.

It might be granted, that this absence or diminution of pressure might occur in certain situations; but how does it happen that this indispensable condition should invariably be found in the subterranean regions, when heat is to be applied to vegetable matter. If the usual pressure were present, that matter, according to Dr. Hutton, would remain unchanged with respect to composition by any heat; but, in nature, we meet with no collections or strata of vegetable matter in this peculiar state, in which they have suffered heat without being changed, yet, surely, since heat is applied to all the other strata under immense pressure, it ought also to have been occasionally applied under a similar pressure to the matter of coal. The key to this apparent mystery is, however, easily found. The common strata are supposed to have been heated under an immense pressure, which is not in any case supposed to have been absent, *because* the presence of that pressure is necessary, in many

cases, to obviate certain objections to the supposition of their igneous origin, and, in all, an uniformity is, if possible, to be preserved. The matter of coal, again, is supposed to have had heat applied to it *always* under a diminished pressure, *because* that circumstance is necessary, in the Huttonian theory, to account for its formation. The bare statement of these accommodating coincidences is sufficient to prove that they are merely fictitious, and that these are suppositions arbitrarily made, because they are necessary in the theory.

Another difficulty may be stated against this hypothesis of the formation of coal. Pyrites, or sulphuret of iron, is supposed to be formed by heat, and this heat is supposed to have been applied under such a pressure, as has prevented the sulphur, which is a very volatile substance, from having been driven off from the iron. Since pyrites occur, therefore, so abundantly in coal, a dilemma is presented to the Huttonian geologist. If he suppose the vegetable matter of coal to have been fused under an immense pressure, capable of resisting the escape of its volatile parts, he contradicts the author of the theory, who tells us, that under such a pressure, that matter could not have been formed into coal, a proposition which, according to his hypothesis, appears to be perfectly just. If he sup-

pose the heat to have been applied under a diminished pressure, so as to allow of the decomposition of the vegetable matter, and the expulsion of its volatile principles, he will have the task of explaining how, in such a situation, pyrites could be formed and crystallized. Nay, the difficulty is still greater, for this substance is found in that species of coal which is little inflammable, and which, according to the Huttonian theory, is supposed to have had heat applied to it under so little pressure, that all its bituminous matter had been expelled. Mr. Kirwan relates, that the Kilkenny coal, which is of all others the most completely destitute of bituminous matter, contains pyrites*; and Dr. Hutton himself mentions a specimen being in his possession of plumbago, (which he considers as the last of the series, or as coal completely deprived of bitumen) studded with pyrites†. The explanation of these appearances, according to the Huttonian system, involves a direct contradiction in terms. To account for the formation of this species of coal, it is said to have been fused with an entire absence of pressure, so that all its bituminous matter has escaped. Pyrites, again, is a substance said to be formed by fusion, but under a strong pressure, by which its sulphur, a

* Geological Essays, p. 473.

† Theory of the Earth, vol. i. p. 616.

substance at least as volatile as bitumen, is kept in combination with the iron. It would, therefore, according to this theory, be impossible that coal of this kind and pyrites should exist together, the circumstance supposed necessary for the formation of the one, being that which must inevitably have destroyed the other.

A similar argument, equally forcible, may be deduced from the connection of limestone with coal. No arrangement is more common in geology than that of limestone alternating with coal, or covering it. The formation of limestone, in the Huttonian system, is ascribed to fusion, under vast compression, by which the carbonic acid has been retained in combination with the lime; and the advantage which the admission of this modifying circumstance gives to the Huttonian geologist in his reasonings, is very amply, perhaps ostentatiously displayed in the defence of the system. It is always admitted, however, that but for this circumstance of compression, the consolidation of limestone by fusion could not be accounted for. The formation of coal, on the other hand, it is contended, cannot take place but when pressure is diminished or withdrawn. When limestone covers coal, it can have been consolidated only by the heat operating through the coal. But if pressure were present on the limestone above,

how could it have been absent from the coal beneath? To suppose this is a palpable absurdity; and therefore, were the Huttonian theory true, either the coal should not have been formed, or the limestone should have been converted into lime.

Among the SALINE SUBSTANCES, Rock Salt is adduced as affording an argument in favour of the Huttonian theory. This substance is found generally in strata, and it is perfectly compact and indurated. This state, it is said, could not be produced by crystallization from water; a mere assemblage of loose crystals, without solidity or cohesion, only could be formed; and to convert these into a firm and solid rock, would require the application of such heat as was able to reduce it into fusion. "The consolidation of rock salt, therefore, cannot be explained but on the hypothesis of subterraneous heat*."

The Neptunist will find no great difficulty in obviating this argument. If the crystallization has, from alteration in the circumstances of the solution, been hasty, it is conceivable that, instead of an assemblage of small regular crystals, large and compact crystalline masses might be formed; or even if the salt first deposited had

but that even much of the muriate of soda now found in sea water, may have been redissolved from strata formed. Or we may modify this explanation, by the equally probable supposition, that at first the soda and the muriatic acid of the salt had not been in combination, but that, in the course of the various alterations of attractions from the precipitations of the strata, they had been brought together, had united, and if locally accumulated, crystallized. Such a supposition receives confirmation from the fact, that in many of the strata, in trap for example, according to the excellent experiments of Dr. Kennedy, both soda and muriatic acid exist; and sea salt itself is found sometimes among primary strata. This general hypothesis, modified in either of these ways, involves no improbable suppositions, and is perhaps adequate to the explanation of the production of this fossil.

Another saline substance, a variety of carbonate of soda found in Africa, is stated by Dr. Hutton as affording a proof of consolidation from fusion. This substance is supposed to have been part of the contents of a vein, as it has a stony crust adhering to one side of it; it has a sparry structure, and contains only about one-sixth of the quantity of water of crystallization contained in the usual crystallized state of this salt. It is this last circumstance which is con-

origin of rock salt; but there are no appearances in this fossil inconsistent with the supposition of its aqueous origin. The Neptunists have supposed, that it may have been formed by collections of the original sea water in hollows, among the strata, having suffered evaporation during the temporary retreat of the ocean, and that these having been successively filled, have furnished the strata of rock salt. But it is a strong objection to this, and all other theories which derive it from the evaporation of sea water, that it does not contain the saline substances which are dissolved in that water, or at least does not contain them in the due proportion, and that it contains fewer remains of marine animals than we should expect, had it had such an origin. Perhaps we may suppose, that the saline substances, in common with others, had existed in the original fluid in which all the materials of the strata were dissolved,—that part of these being locally accumulated, in the same manner as the materials of every other stratum have more or less been, from circumstances which we cannot determine, had been confusedly crystallized; and that any remaining portion had been retained in solution by the water, these salts of course remaining in it, in increased proportion which were least disposed to crystallize. Such is the muriate of magnesia; and it is not impossible

every other substance which the sea water had held in solution ; or rather, if it ever had been brought to the sea, it must have immediately been decomposed by the muriate of magnesia present, and could never have existed in the waters of the ocean. The Huttonian theory is therefore actually incapable of affording a supposition, by which carbonate of soda, as a fossil substance, could be formed. Mr. Kirwan adds, as a proof that it has not been fused, its containing some grains of sand, which would necessarily, in such a case, have been vitrified. And were it supposed to have been in fusion, it would require to be explained how the pressure was so nicely adjusted as to admit of the greater part of its water of crystallization being driven off, while none of its carbonic acid, even though it be supersaturated with it, and of course retains the excess by a very weak affinity, had been expelled.

The production of this salt, if it really be part of a vein, may be explained on the Neptunian system by a similar hypothesis to that proposed ; to account for the formation of rock salt, and from its hasty crystallization, or its super-saturation with carbonic acid, (which, from its analysis, is found to be the case,) the diminished quantity of water of crystallization in it may be accounted for. If the fact, however, be true, which Bergman states, that the carbonate of so-

da found in the earth, both in India and Africa, is free from common salt at the surface, but becomes contaminated by it as it descends deeper, it is probable that this salt may have been formed at the surface from the decomposition of muriate of soda by some unknown power, and from this mode of formation its containing less water of crystallization might arise.

In concluding this argument with regard to the saline substances, the formation of one of them which is not noticed in the Huttonian geology, gypsum or sulphat of lime may be stated. This salt is soluble in 500 times its weight of water; of course, had it existed in the ancient world, and been brought by disintegration to the ocean, it must have remained dissolved in the water till that fluid was saturated with it; and there probably does not exist in nature a quantity sufficient for that purpose. It would then remain to be explained how it could be collected in particular places, and be precipitated. The theory, applied to the formation of sea salt, will here be of no avail; for should we suppose it present in a large proportion in sea water in a particular place, the evaporation of that water could not account for its consolidation, since it must have been accompanied with sea salt, a substance of which, according to the experiments of Saussure, gypsum frequently does not

contain an atom. This, therefore, is another fossil, for the formation of which the Huttonian theory cannot account. Its origin, on Neptunian principles, may probably be explained in the same manner as that of rock salt.

The state in which the METALS are found in nature, either pure or combined with other substances, appears to afford a strong argument in favour of the Huttonian hypothesis; for the fluidity from which they have consolidated may have been produced by fusion, but we scarcely can point out by what solvent it could have been effected. Gold, silver, copper, and some others, are frequently found native or uncombined. "Of all such specimens it may be safely affirmed, that if they have ever been fluid, or even soft, they must have been so by the action of heat; for to suppose that a metal has been precipitated pure and uncombined from any menstruum is to trespass against all analogy, and to maintain a physical impossibility*."

The assertion in this paragraph is much stronger than what the facts can establish; for although it may be difficult to point out the mode in which metals have actually been precipitated, yet their precipitation from any menstruum, so far from being a physical impossibility, is what happens

* Illustrations, &c. p. 59.

every day, and can be effected at pleasure. Metals in a state of combination with acids, and in solution in water, are thrown down in their metallic state by each other, by hydrogen, sulphurated hydrogen, and various other inflammable bodies. The Neptunian will readily acknowledge, that it is extremely difficult to point out, even by hypothesis, by what particular agency the metals found in nature had been dissolved and precipitated, but at the same time he has ample demonstration, that it is in this way, and not by fusion, that they have been formed.

This proof is obtained from the crystallized state in which they are frequently found. "Specimens of quartz, containing gold and silver shooting through them, with the most beautiful and varied ramifications, are every where to be met with in the cabinets of the curious, and contain in their structure the clearest proof that the metal and the quartz have been both soft, and have crystallized together *."

It may be pronounced a physical impossibility, that from simple fusion quartz and gold, or quartz and silver, could crystallize together so as to exhibit these appearances. These metals are fused at a heat, which, compared even with

* Illustrations, &c. p. 59.

the degrees of heat we have it in our power to produce, may be termed very moderate, while quartz we are unable to fuse. If, therefore, both substances were in fusion, on a reduction of temperature, the quartz must have become solid long before the metal; and it is absolutely impossible that the metal could have shot through the quartz. Or, to place this in a point of view more precise, gold melts at a temperature equal to 32 of Wedgewood's pyrometer, and at all temperatures above this it must remain fluid: Quartz does not melt at the highest heat that has been accurately measured; but, according to the experiments of Saussure, it is not less than 4043 of Wedgewood's scale, and consequently at every temperature below this, must continue solid.—Grant, therefore, to the Huttonian, that both quartz and gold were in fusion, it is evident that on a reduction of temperature to 4000, the quartz would become solid, or crystallize; but it is equally certain, that at this temperature, and for more than 3900 lower, the gold must remain fluid: the supposition, therefore, that the gold could become solid, and shoot through the fluid quartz, involves a direct contradiction in terms, or supposes a physical impossibility; and of consequence, the various appearances in these specimens which prove that the metal and quartz had crystallized together, or that the former had

crystallized first, prove, as much as any phenomena can, that these crystallizations could not be from simple fusion. It will not surely be presumption to affirm, that if this be not admitted as an undeniable conclusion, as affording even a demonstration as certain as any can be, all reasoning on the subject must be given up, for it is impossible to conceive a proposition more evident, or the reverse of which involves a more palpable contradiction in terms. The statement of these facts, as favourable to the Huttonian system, affords a striking example how far the mind may be misled by a favourite hypothesis, the very appearances which prove its falsity being adduced as proofs of its truth.

One mode might perhaps occur in which it might be attempted to remove this difficulty. It may be conceived, that the quartz had first become solid, and that merely the fissures of it had been filled with crystallized gold. But such a supposition has been very clearly refuted by Mr. Playfair himself. " Between the channels
 " in which the metal pervades the quartz, and
 " the ordinary cracks or fissures in stones, there is
 " no resemblance whatever; a system of hollow
 " tubes winding through a stone (as the tubes
 " in question must have been before they were
 " filled by the metal), is itself far more incon-
 " ceivable than the thing which it is intended

The argument, *a priori*, that pyrites cannot be formed in the humid way, because neither the sulphur nor the metal is soluble in water, is founded on an erroneous conclusion; for, granting the fact, each might have been combined with other substances which would render them soluble, and, in this state of solution, they might, from the chemical affinity subsisting between them, leave the substances with which they were united, and combine together. This actually happens in the example of sulphurated hydrogen, with a number of the metallic salts or oxyds. If this compound of hydrogen and sulphur, be introduced into a solution of any of the salts of lead, the hydrogen combines with the oxygen of the metallic oxyds, and the lead combines with the sulphur, forming a compound which, according to the observation of Vauquelin, has all the properties of galena. It is not even necessary that the metal should be combined with an acid, for if humidity be present it will be oxydated, and upon this oxyd the sulphurated hydrogen is capable of acting and producing a metallic compound, or the same substance is even capable of acting on the pure metals.

This argument must be admitted as perfectly conclusive, when it is proved, that pyrites,

and other compounds of the metals with sulphur, are formed by nature in the humid way, and of this the proofs are abundant.

Thus pyrites is found in situations which clearly indicate its aqueous formation. It is associated with calcareous crystals lining the internal cavity of shells, which could not have been in fusion, since the heat necessary to fuse the pyrites, or the calcareous crystals, must have destroyed the texture of the shell,—it is often to be traced in the impressions of organic substances, particularly of animal remains in coal and other fossils; it exists in bituminated wood, which, it must be granted, could not have been fused; and it has been found forming on the surface of wood, in mines, and in other situations, which establish the same conclusion, of which several examples are stated by Mr. Kirwan*.

The observations of Mr. Wiseman on the effects of the waters of the Mere of Difs on metallic substances; and the farther experiments and observations of Mr. Hatchet, on the same subject, are also particularly valuable in proving the humid formation of these compounds. Mr. Wiseman observed, that flints, and other stones, immersed for some time in this stagnant water,

* Geological Essays, p 400

were incruſted with a metallic ſtain. This, by analyſis, was found to be fulphuret of iron ; and when copper was kept in this water, it was encruſted by a ſubſtance which was found to be compoſed of 70 parts of copper, 16.6 of ſulphur, and 13.3 of iron. This encruſtation was even found in a cryſtallized ſtate. Theſe experiments on this ſubſtance, and on the encruſtation of martial pyrites on the flints, were confirmed by Mr. Hatchet. With reſpect to the latter, he obſerves, there could be no hesitation ; and the former he conſidered as “ in every property “ ſimilar to that rare ſpecies of copper ore, called by the Germans *Kupfer ſchwärtze*, (cuprum ochraceum nigrum) and abſolutely the “ ſame.” At the deſire of Mr. Hatchet, ſilver was immerſed in this water, and it was found to be encruſted with a ſubſtance “ ſimilar in every “ reſpect to the ſulphurated or vitreous ore of “ ſilver, called by the Germans, *Glaſertz*.” The ſame diſtinguiſhed chemiſt adds, that effects ſimilar to theſe, on a larger ſcale, there is reaſon to believe, “ have been, and are now daily produced in many places. The pyrites in coal “ mines have probably, in great meaſure, thus “ originated. The pyritical wood may thus “ have been produced ; and by the ſubſequent “ loſs of ſulphur, and oxydation of the iron, this

"pyritical wood appears to have formed the
 "wood like iron ore, which is found in many
 "parts*." Of the formation of metallic sulphurets in the humid way, there can therefore be no doubt, and instead of having any difficulty to encounter in explaining the origin of these substances, the Neptunist may justly bring them forward, as affording a proof of his theory, and a proof of considerable importance, from the extensive connections they have with other fossils.

If it were necessary to say more on this subject, which perhaps it is not, it might be remarked, that from the manner in which these compounds are associated with certain fossils, we have the same demonstration as in the example of the pure metals, that they cannot have been formed by fusion. Not only the sulphuret of iron, but those of antimony, mercury, and silver, are frequently found crystallized or disseminated through quartz and other fossils. Now these sulphurets are very easily fused, while these fossils are comparatively infusible; it is therefore impossible that the former could have crystallized within the latter, or been disseminated through them, since, to admit of its regular crystallization, or even of its dissemination in another

presumption, that both had consolidated from fusion? Yet the fact, as it is, is still stronger in proving, that such could not have been their origin, since it is an evident impossibility, that, in two substances fused together, the most fusible should concrete before the one that was least fusible, or required the greatest heat to keep it fluid. Had the former even been congealed by some cause, of which we can form no conception, the high temperature keeping the other, fluid, must have again immediately fused it.

Under this head may be noticed several facts, affording an argument of a similar kind ;—those in which fossils are regularly crystallized, in others which are much less fusible. Thus besides the crystallization of felspar in granite, regular crystals of it are not unfrequently found embedded in quartz, a proof which cannot be eluded, that the felspar has become solid, while the quartz remained fluid, contrary to what must have happened from the known fusibilities of these substances, if they had consolidated from fusion. Shorl is a substance of comparatively easy fusibility, yet it is often crystallized in quartz, the fibres of the shorl being finer even than the human hair, shooting through a large mass of quartz in every direction, and with various wavings and incurvations, so as to prove decisively that the quartz had been completely

liquid when the short crystallized. Asbestos is another substance found shooting in the most delicate fibres through quartz, though it melts at 378 of Wedgewood's scale. And, without enumerating more examples, micaceous schistus is a rock of very difficult fusion, yet it is the common matrix in which garnets are enveloped, and though these are much more fusible, they are crystallized in its substance with the greatest regularity. It must be superfluous to repeat the argument from such facts. If they do not prove that these fossils have not been formed by fusion, no conclusion can be established in geology, and we may relinquish every attempt to theorise.

WHIN OR TRAP is a rock into which granite insensibly graduates, so that what is proved with respect to the one, may be considered nearly as proved with regard to the other. There are some particular facts, however, with regard to whin, which are supposed by the defenders of the Huttonian theory to prove its igneous origin.

Whin, it is said, resembles lava in its appearance so much, that some varieties of it have been mistaken for volcanic products. This resemblance " leads to suspect, that the two stones
" have the same origin, and that as lava is cer-
" tainly a production of fire, so probably is

“ whinstone *.” Any diversity existing between them consists principally in whinstone containing calcareous spar, which lavas do not ; and this diversity is likewise explained in the Huttonian system ; as this substance might be formed in whin, from the fusion of it having been under an immense pressure, while lava, being in fusion exposed to the air, it must have been decomposed. “ Thus,” it is added, “ whinstone is to be “ accounted a subterraneous or *unrupted* lava ; “ and our theory has the advantage of explaining both the affinity and the difference between these stony bodies, without the introduction of any new hypothesis. In the Neptunian system, the affinity of whinstone and “ lava is a paradox which admits of no solution †.”

To this argument it may be replied, that the mere resemblance in appearance between different fossils is a very weak proof of a similar origin, since in many cases close resemblances are to be traced between fossils altogether different in their nature. But were it a just conclusion in general, in the present case it is not so, because the resemblance can be otherwise very easily explained. It is extremely probable that lava

* Illustrations, &c. p. 68.

† Illustrations, &c. p. 69.

is formed from rocks of the nature of whin, fused by the volcanic fire. It is apparent, from the descriptions of Spallanzani, and other mineralogists, that rocks of this species are the basis of volcanic countries: zeolite, leucite, and other fossils, usually found in rocks of this order, are likewise contained in lavas, altered, but, according to the opinion of the best mineralogists, not formed by the volcanic fire. And, lastly, the excellent analyses of Dr. Kennedy prove the near resemblance in composition of trap and lava. They show, as he himself observes, "that whins, and a certain class of lavas taken from remote quarters of the globe, consist of the same component elements united in each, nearly in the same proportion. The only circumstance in which they materially differ, is the loss of some volatile matter in the fire, which is peculiar to the whins alone." The conclusion, therefore, evidently amounts even to more than a high probability that lava has been formed from the fusion of trap; and if this be true, the resemblance between them is no proof whatever of their having had a similar origin. Whatever may have been the origin of the trap,—although it be of aqueous formation, it is perfectly conceivable, that if fused, as none of its principles are lost, it might form a sub-

stance very similar in its properties to the trap in its original state. It is equally evident, that the difference between trap and lava, that of the latter containing no carbonate of lime, is on this supposition fully explained, since, if the trap were fused in an open volcanic fire, the carbonic acid would be expelled from the lime. The affinity, therefore, between lava and whinstone, in some points, and their difference in others, are fully accounted for, and cannot be regarded as forming, "in the Neptunian system, a paradox which admits of no solution."

But there is a difference between these substances not noticed by the Huttonian geologist, and for which he will find it difficult to account. We are told that trap differs in lava, in nothing but in the circumstances of their formation, the one having been melted matter erupted at the surface while fluid, the other having been thrown up among solid strata, and consolidated under an immense pressure. Hence is explained the presence of carbonate of lime in the one, while it is not found in the other; and it follows, from the opinion itself, that lava and trap should differ from each other in nothing but in such properties or appearances as are capable of being produced by the cause thus specified. Will the Huttonian geologist then inform us why agates,

and masses or veins of quartz, or even regular crystals of it, which are abundant in basalt, are not to be found in lava? The absence of compression could not prevent their formation, or render them more ready to be destroyed if they were formed; and it is evident that lava and trap ought, according to the Huttonian theory, to differ in nothing but what this absence of compression in the one case could occasion.

The columnar structure which the close-grained whin sometimes assumes, forming basalt, has been supposed a proof of its igneous origin, because the same structure, it is said, is sometimes assumed by the lava actually erupted from volcanos. It is to be observed, that in the greater number of lavas, cooled under every variety of situation, either slowly by exposure to the air, or rapidly by having flowed into the sea, this columnar appearance is not to be observed; and many of the instances which have been given are extremely doubtful, from rocks not volcanic having been so often confounded with lavas. If it be admitted that real lavas do sometimes assume a columnar form, (and such instances, if they exist, are extremely rare,) still these facts clearly show that this has not been an effect arising from their species of fluidity, or their mode of consolidation, as in by far the greater number of cases in which these causes must have

equally operated, it is absent. Did it arise, indeed, from either of them, it ought to be observed in almost all the varieties of whin, and might even be expected in the other unstratified rocks. It may therefore be ascribed with more probability to another cause ;—the peculiar nature or composition of the matter of which lava consists : And if this, as there is every reason to believe, be the real cause, it is evident, that since basalt is perfectly similar in composition to lava, it might assume the same form in its consolidation from an aqueous origin : that, in short, if the form do not originate from the mode of consolidation, but from a property belonging to the substance itself, and originating in its composition, it may equally be exhibited by lava becoming solid from fusion, and basalt becoming solid from solution. It is not improbable that this property may arise chiefly from the predominance of argil in these fossils, as it is found to take place even in other argillaceous fossils, as in the argillaceous iron ore, some varieties of marl, and even some of argillaceous sandstone,—substances, some at least of which have evidently never been fused.

“ A mark of fusion, or at least of the operation of heat, which whinstone possesses in common with many other minerals, is its being penetrated by pyrites—a substance, as has been

already remarked, that is of all others most exclusively the production of fire *." The superior probability of pyrites being of watery origin has been already shown, and of course this fact becomes a strong argument for the aqueous formation of whin. A similar argument is that derived from the presence of agates in trap, these fossils being supposed to give indications of having been formed by fire. This supposition has already been considered, and the conclusion endeavoured to be proved, that, in common with other fossils, they owe their formation to the agency of water.

The last argument for the igneous origin of whin, which has not been noticed, is that deduced from the experiments of Sir James Hall. It had been often stated, as an objection to the opinion that basalt was of igneous origin, that it ought to have a vitreous lustre and fracture, since from melting any earthy combination some species of glass is always produced, and since basalt itself by fusion actually forms a real glass. Sir James Hall, by a number of excellent experiments, has clearly shown, that when this stone is brought into fusion, if the refrigeration of it be very slow, it assumes the stony character, or is scarcely distinguishable

* Illustrations, &c.

from the real basalt, and that it is only when hastily cooled that it acquires any of the properties of glass.

By some of the defenders of the Huttonian theory the conclusions from these experiments have been carried much farther than they warrant, or than their author has stated. Because from the fusion of trap or basalt a substance similar to these fossils was by particular management produced, it has been concluded that this affords a proof of fusion being the means of their first formation. This conclusion is an evident mistake: the fusion of the basalt, as it contained no volatile substance of importance, could not alter its composition, and of course when it again became solid, it would still be endowed with its former properties. Even if the simple earths of which basalt consists had been mixed together in the proportions which its analysis affords—if by fusion these had been brought into union, and formed a substance similar to natural basalt, still this would not have proved the igneous formation of this fossil; for as all compounds derive their properties from their composition, if their constituent parts are capable of being brought into union by the humid way, and also by fusion, it may be expected that a similar compound will in both cases be formed; and the actual production of such a compound

by one of these modes, would not prove that it was incapable of being formed in the other. Had therefore this experiment been made, no conclusion of this kind could have followed, still less can it be inferred from merely fusing natural basalt, and finding that by slow cooling a substance similar to it is formed. In strict reasoning, this experiment adds nothing positive to the evidence of the Huttonian system; it only removes an objection which could have been urged with justice against it,—and in this point of view it may be prized by the defender of that doctrine.

It is remarked, that “ the experiments of another ingenious chemist, Dr. Kennedy, have shown, that whinstone contains mineral alkali, by which of course its fusion must have been assisted*.” It may be added, that the presence of this alkali would not less contribute to the solubility of the matter of whin in water.

Besides the proofs of the aqueous origin of whin, which have been noticed in the course of this argument, it may be added, that there are facts which indisputably establish it. These are, the existence of foreign bodies in it, which could not possibly have been present had it been thrown up in a state of fusion from the

* Illustrations, &c. p. 80.

bowels of the earth. Mr. Jamieson has given a very full enumeration of these, on authorities which it would be presumption to dispute. Thus, Werner found in wacken, great trees with branches, leaves, and fruit, and observes, that it is sometimes found to contain deers horns. Saussure observed in it bones of quadrupeds; and other mineralogists have found shells, vegetable impressions, and fragments of wood. It has been stated with respect to some of these, that they are found, not in the basalt, but in strata which alternate with it; and in some cases this appears to have been the case. But in the observations of Werner and Saussure we are not at liberty to suppose that they committed so obvious a mistake; and therefore these observations are decisive proofs of the aqueous origin of these rocks.

The properties therefore of the substances composing the unstratified rocks equally with their positions exclude the operation of fire, and prove them to be of aqueous origin.

We have thus completed the examination of the HUTTONIAN and NEPTUNIAN theories; and it cannot be difficult to form an opinion on their merits. To the Huttonian system belongs the praise of novelty, boldness of conception, and extent of views. Its author has aspired not merely to ac-

count for the present appearances of the earth, but to trace a system in which the formation of successive worlds is developed; he has sought to extend that order and arrangement, that principle of balance and restoration observed in all the departments of nature, to the constitution of the globe itself; and he has succeeded in drawing an outline which gratifies the imagination with the semblance of grandeur and design.

But these are the only merits of the theory, and they have certainly been much over-rated by the partiality of its defenders. When full scope is given to the imagination, when the restraint of strict induction is not imposed, experience, and especially experience in geology, has shown, that it is no difficult task to construct a system, and to give it in appearance that unity of principle, and adaptation of parts which are the attributes of a perfect theory. This may be what the author of the Huttonian system has attained; but a more just and cautious reasoner would have startled at the *postulata* the system requires, and would not have thought their extravagance and improbability, their inconsistency with phenomena, and their opposition to established truths, compensated by the greatness or novelty of its views.

It can scarcely be necessary to justify these observations by any recapitulation of the evi-

dence of this theory. In appealing to the proof from induction, we have found the phenomena of geology entirely at variance with its principles. It has not been necessary to search for minute deficiencies, or select a few conclusive arguments from a number that are doubtful or obscure ; the whole series is clear and convincing ; the positions and relations of the great masses of the globe, and the properties and appearances of individual fossils, being altogether incompatible with the supposition of their having been formed by a central fire. Its first principles we have found not merely in the highest degree improbable, but absurd and physically impossible. It assumes the existence of an intense heat in the interior parts of the earth, without assigning any cause by which it could have been produced ; it supposes an exertion of that heat, not merely at the formation, or during the period of the existence of a world, but for a time absolutely unlimited ; and it advances these suppositions in express contradiction to the known and established laws of the power it employs. Such characters bring the Huttonian hypothesis under the same class with those geological systems which have preceded it,—systems which have been the meteors of their day, and have sunk into obscurity ; and which, to use the language of Dolomieu, will never be mentioned

in the history of the science, but as pointing out the by-paths in which those may wander who devote themselves to the contemplation of nature.

With this system, the Neptunian theory forms a complete contrast. It presumes not to carry its researches past the commencement of the present world, or to extend them beyond its termination ; it is satisfied with endeavouring to trace the causes of the appearances which at present exist ; and the characters of its explanations are these of fair and legitimate deduction. All the phenomena of geology conspire to prove that water has been the great agent by which minerals have been formed, and the surface of the earth arranged. While the science remains in an imperfect state, deficiencies must be found in the application of this principle which induction establishes. Such there may be in the Neptunian theory ; and that there should, is even a presumption of its truth. But we discover no inconsistencies with that principle, nor contradictions to known truths. We find in it, in reality, what we should at present expect in a just theory of the earth : not the magnificent pretensions of an artificial system,—not the splendid illusions created by a bold imagination, but a series of inductions more or less perfect, referred to a common principle, and occasional-



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